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PROCEEDINGS

OF

THE ROYAL SOCIETY.

"On a Colloid Acid, a Normal Constituent of Human Urine." By William Marcet, M.D., F.R.S. Received May 28, 1864*.

In the autumn of 1862, feeling assured that, besides the known normal crystalloid compounds found in urine, this secretion contained colloid substances, I submitted samples of the healthy secretion, after concentration, first, to the process of dialysis, and then to the action of reagents, and finally succeeded in precipitating with alcohol a colloid substance exhibiting a faintly acid or neutral reaction, and containing a small proportion of ash. For a while my endeavours to obtain a definite compound from this amorphous mass were fruitless, until, on observing that basic acetate of lead produced a precipitate in its aqueous solution, I thought of examining this precipitate, and, by decomposing it with sulphuretted hydrogen, found it to consist of an organic acid combined with lead. This new acid is possessed of the properties of a colloid substance; it may be considered as having a definite combining proportion or equivalent weight, and is undoubtedly destined to become of great importance in physiological chemistry.

After having satisfied myself of the presence of a colloid acid in urine, I tried every means to obtain as much of it as possible from a given volume of the secretion, and prepare it in the pure condition; and after having experienced many difficulties, I adopted the following method as the simplest and that yielding the most satisfactory results.

Mode of Preparation of the Colloid Acid.

Urine is mixed with animal charcoal and concentrated until reduced to about one-fourth of its original bulk. It is filtered, and a solution of caustic baryta is added until complete precipitation by this agent be effected. The fluid filtered from the baryta precipitate is dialyzed without further evapo-

* Read June 16, 1864: see Abstract, vol. xiii. p. 314.

ration, for a period of about forty hours; after which it is again concentrated, filtered, and then a solution of basic acetate of lead is added to it, which produces a precipitate: care should be taken not to use more of this solution than is necessary to obtain a complete precipitation in the fluid. The precipitate appears white, although containing a little colouring-matter; it is to be collected on a filter and washed with distilled water until the washings contain but a trace of lead. The insoluble substance on the filter is a lead-compound of the organic colloid acid; it still contains a small quantity of colouring-matter, some hydrated oxide of lead, more or less chloride of lead, and perhaps traces of sulphate of lead. In order to effect the removal of these impurities, the insoluble compound is first decomposed with sulphuretted hydrogen or sulphuric acid; if sulphuretted hydrogen is employed, the excess of this gas is afterwards expelled by boiling, or better by blowing it out with a current of air.

The acid fluid being now heated with animal charcoal and filtered, loses the whole, or nearly the whole of its colouring-matter, and apparently much of its urinous smell. Any hydrochloric acid present can be easily separated from the free organic acid by treating the colourless fluid with carbonate of silver and filtering; the dissolved silver is afterwards eliminated by means of sulphuretted hydrogen, and the excess of sulphuretted hydrogen again removed by boiling the fluid, or passing through it a current of air. The careful addition of baryta-water will precipitate any sulphuric acid present. Finally the acid is precipitated afresh with basic acetate of lead, from which it is separated by decomposing the thoroughly washed precipitate with sulphuretted hydrogen.

The object of the various operations thus described will be readily understood. By evaporating urine with animal charcoal, it is first partly discoloured; the precipitation with baryta-water throws down the phosphoric and sulphuric acids and the lime of the secretion, which would interfere with further operations, and imparts to the fluid a strongly alkaline reaction, this last condition being apparently necessary to avoid a loss of the colloid acid during the dialysis. In order to prevent decomposition, which would occur by concentrating with heat a strongly alkaline urine, the fluid is dialyzed at once for about thirty-six hours, which operation removes from it a considerable proportion of its crystalloid constituents; among these are chlorides, which it is advisable to get rid of, as much as possible, before concentration and precipitation with basic acetate of lead, because by so doing a great saving in the carbonate of silver, necessary to precipitate the remaining hydrochloric acid, will be effected.

If basic acetate of lead is used in excess, the precipitate begins to redissolve; the precipitant should therefore be added very gradually, testing the fluid now and then to ascertain whether the precipitation be complete. I have also observed the precipitate to be soluble in a solution of caustic potash. The lead-compound collected on a filter is to be thoroughly washed, to remove any excess of basic acetate of lead, and a solution of the lead-compound of the colloid acid, to which I shall refer hereafter.

As the lead precipitate appears to be slightly soluble in water, it is difficult to ascertain precisely the period at which the washing may be considered sufficient. I usually continued pouring distilled water into the filter until sulphuretted hydrogen gave but a faint dark colour in the filtrate. The decomposition of the insoluble compound by means of sulphuretted hydrogen is a slow process, and takes some hours before it is complete; and it will be advisable to leave the precipitate in contact with sulphuretted hydrogen overnight: the excess of sulphuretted hydrogen may be expelled by boiling, or by a stream of air, by which latter method the coloration caused by heat is avoided.

The Acid.—The fluid prepared as stated above has a strong acid reaction; exposed to the air for a fortnight, and even for a longer period, it is not altered. When concentrated even by very brisk boiling, it may be considered as undergoing no loss and no decomposition, as shown by the following experiments:—75 cub. centims. of the acid, 5 cub. centims. of which were neutralized by 24·3 cub. centims. of a normal potash solution, were boiled down to 15 cub. centims., then diluted with water to 75 cub. centims., and tested with the normal potash solution; 23·4 cub. centims. of this solution were found necessary to neutralize it. Any decomposition must have been very slight, as the difference between the volumes of the potash solution added before and after the boiling was only of about 1 cub. centim.

As a further proof that the acid is not volatile, I distilled a sample of it in a retort, in the free flame, carrying on the operation until nothing but a dark semifluid mass remained in the retort. The distillate was just faintly acid to the most delicate test paper. In most of my experiments the acid was slightly coloured, and evolved a urinous smell when hot. Under the impression that this odoriferous substance might be a volatile acid, such as those discovered by Städeler *, I was led to examine very carefully the distillate obtained as above. This fluid had a strong smell of urine and the faintest acid reaction. After the fluid was rendered alkaline by baryta, the smell was in no way diminished; so that it could not be owing to an acid; moreover, considering that the colloid acid loses its colour, and apparently in a great measure its odour, after agitation with animal charcoal, we may infer that the odour of urine is owing to a very slight decomposition of the colloid acid which takes place under the influence of heat, and more especially in the presence of free mineral acids.

Returning to the solution of the colloid acid in water: after concentration by heat its colour darkens and it becomes syrupy, with a sharp acid taste, and a slight acrid and astringent after-taste. This taste is perceptible in the solution, even when very dilute. I could never obtain any crystals in this syrup beyond those resulting from inorganic impurities. When dried, the acid assumes the form of a transparent varnish, which, by a temperature of 120° Cent., becomes much darkened. The dried

* Annal. der Chemie und Pharm. vol. xcvii. p. 134.

substance is very hygroscopic, and dissolves readily in water (with the exception of some few dark flakes) after exposure for some time at 120° Cent. Alcohol, sp. gr. 827, gives it a dull, opaque appearance, and slightly dissolves it. The dry acid is insoluble in ether, and its solution in diluted alcohol is rendered turbid by ether. When burnt it chars, emitting a pungent smell; the ignition is attended with but a very faint flame, showing that very little hydrogen enters into its composition; nothing but a trace of fixed inorganic residue remains after complete incineration of the acid. The colloid acid was found to have no action on polarized light; it failed to precipitate egg-albumen, but precipitated casein in milk; the precipitate was not redissolved in an excess of the acid, as in the case of acetic acid; although strictly a colloid, it passes through the diaphragm of a dialyzer, but the phenomenon is not near so rapid as in the case of crystalloids. In an alkaline fluid, however, the acid (under the form of a compound) does not find its way so readily through the dialyzer, and its passage is thereby checked in a considerable degree.

The qualitative composition of the colloid acid of urine was obtained by subjecting to analysis its insoluble lead-compound. I found the organic substance to consist only of carbon, hydrogen, and oxygen. I have not yet determined the ultimate quantitative composition of the acid, but have succeeded in showing that it possesses an atomic weight, or combining proportion, thereby proving the acid to be a definite substance; the atomic weight of the acid was determined by the analysis of its insoluble lead-salt and of its baryta-salt. In order to analyze the insoluble lead-salt, a weighed quantity of the compound, dried at 120° Cent., was dissolved in acetic acid, and precipitated by means of sulphuric acid, with the addition of alcohol; the sulphate of lead was collected in a filter, the filter burnt, and the inorganic residue treated with sulphuric acid; the sulphate of lead was finally weighed.

TABLE showing the results obtained from the analysis of the insoluble lead-salt of the Colloid Acid of Urine.

Six samples of the acid.		Weight of lead-compound analyzed.	Weight of protoxide of lead found.	Protoxide of lead in 100 parts of the compound.	Acid in 100 parts of the compound.		
Exp.		grm.	grm.				
I.	A.	0·223	0·146	65·5	34·5	Average	66·4
	B.	0·298	0·2006	67·3	32·7		Acid...33·6
II.	A.	0·6575	0·4250	64·6	35·3	Average	65·0
	B.	0·516	0·338	65·5	34·5		Acid...35·0
III.	A.	0·3735	0·2360	63·2	36·9	Average	64·2
	B.	0·2835	0·1850	65·3	34·7		Acid...35·8
IV.......		0·5327	0·3552	66·6	33·4		
V.......		0·612	0·5493	66·1	33·9		
VI.......		0·7183	0·6828	69·9	31·1		

Average in 100 parts { Oxide of lead...... 66·3
Acid .... 33·7

100·0

Five out of the six analyses were made with the acid slightly coloured, to avoid possible inorganic impurity from the use of animal charcoal. In analysis No. VI. the acid had been treated with animal charcoal, and in this case the percentage of lead was a little higher. It is possible that 33·7 is not the exact percentage of acid in the insoluble lead-compound, although it cannot be far from correct; but when the difficulty of obtaining the lead-precipitate in a pure condition is taken into account, I think it will be admitted that the results of these analyses approach each other closely enough to show that the lead-precipitate of the colloid acid is a definite chemical compound. Adopting the number 33·7 as the percentage of acid, the equivalent weight of the acid and of the compound will be

PbO	111·57	
Acid	56·70	atomic weight of acid.
	168·2	atomic weight of compound.

To prepare the baryta-compound of the colloid acid, I began by decomposing with sulphuric acid a known weight of the lead-salt; and by proceeding as in the case of the analysis of the lead-compound, the amount of the colloid acid present was determined. The acid was entirely washed through the filter along with some free sulphuric acid, and I treated the acid filtrate with carbonate of baryta, the mixture being heated for four or five hours, and filtered the next day; the precipitate on the filter was finally washed with hot distilled water for from four to six days, and even after this lapse of time the washings still contained a trace of baryta. I shall only report two out of several of these analyses, as in the others the deficiency of baryta dissolved was obvious. I have calculated the atomic weight of the acid from the baryta-compound which yielded most baryta, as follows—

Analysis No. I.

			per cent.
Compound of colloid acid and baryta 0·6382	BaO	0·4607	72·2
	Acid	0·1775	27·8
			100·0

Analysis No. II.

Compound of colloid acid and baryta 0·9058	BaO	0·5950	65·7
	Acid	0·3108	34·3
			100·0

The atomic weights derived from analysis No. I. are—

BaO	76·39
Acid	29·5
Compound	106·0

If we now compare the atomic weight of the acid in the baryta-compound (29·5) with that in the lead-compound (56·7), it will be readily seen that the relative proportion of these two numbers being very nearly one to two, the lead-compound contains two equivalents of colloid acid ($2\times 28{\cdot}3$), and the baryta-salt one equivalent of the acid (28·3); the insoluble lead-compound is therefore an acid salt of the colloid acid. I shall propose the number 28·3 as the atomic weight of the new acid. We are now able to explain why the insoluble lead-salt of the colloid acid is soluble in an alkaline fluid such as potash. Of the two equivalents of colloid acid, one combines with oxide of lead, and the other with potash, forming two soluble neutral salts,

$$\mathrm{PbO} \,.\, 2\ \mathrm{acid} + \mathrm{KO} = \mathrm{PbO} \,.\, \mathrm{acid} + \mathrm{KO} \,.\, \mathrm{acid}.$$

Compounds of the Colloid Acid of Urine.

Lead-salts.—Basic acetate of lead added to the free acid, or solutions of its neutral salts, gives rise to a white precipitate. When a glass rod moistened with a solution of basic acetate of lead is immersed into a moderately strong solution of the free acid, a precipitate forms, which disappears on agitating the fluid; this can be repeated several times before the precipitate becomes permanent. When the fluid no longer turns clear on agitation, the application of heat will dissolve the precipitate, but on the further addition of the precipitant, the hot liquid will soon remain turbid. An excess of basic acetate of lead redissolves the precipitate; this resolution appears to take place more readily when an excess of the precipitant is added to neutral salts of the colloid acid, than when added to the free acid.

After mixing basic acetate of lead with urine, treated as described above, and filtering, I observed that the filtrate still contained much organic matter, although the further addition of the reagent caused no turbidity. I at first naturally thought that this was owing to the presence of another colloid substance in urine; but my surprise was very great when I found that the pure acid obtained by decomposing its insoluble lead-compound could be but partly re-precipitated by means of basic acetate of lead, a comparatively large portion of organic matter remaining dissolved, as shown by evaporating a few drops of the fluid and incinerating the residue, which charred and burnt away, leaving a little oxide of lead on the spatula. Having previously ascertained that the pure basic lead-salt was nearly perfectly insoluble in water, I could not for some time explain the phenomenon. On reflecting upon the fact, it occurred to me that, as the liquor in which the acid had been precipitated appeared to contain some neutral acetate of lead*, possibly this salt had the power of dissolving the preci-

* The formation of this compound might be considered to have taken place as shown in the following equation:—

$$\left.\begin{matrix}\mathrm{PbO}\\ \mathrm{PbO}\\ \mathrm{PbO}\end{matrix}\right\}\mathrm{C_4\,H_3\,O_3} + 4\ \mathrm{acid} = 2\ (\mathrm{PbO},\ 2\ \mathrm{acid}) + \mathrm{PbO},\ \mathrm{C_4\,H_3\,O_3}.$$

pitate; and on mixing some of the precipitate with a solution of neutral acetate of lead, and boiling, I observed a solution of the colloid compound to take place. This experiment, repeated several times with different samples of the insoluble lead-compound of the colloid acid, yielded invariably the same result: although it happened in most cases that the precipitate was not entirely dissolved, it was very obvious that the greater portion of it had disappeared. The phenomenon in question is very interesting, not only because it accounts partly for the fact that basic acetate of lead does not precipitate the whole of the colloid acid of urine, but also because it affords additional proof of the insoluble lead-compound being an acid lead-salt of the colloid acid. A piece of wet blue test-paper is reddened when held over the opening of a test-tube in which the mixture of the precipitate and neutral acetate of lead is being boiled, showing that acetic acid is given off. This would not happen unless there was an acid present to displace the acetic acid from the neutral acetate of lead; and this acid must be the acid lead-salt. There is now no difficulty in accounting for the solution of the acid lead-salt in neutral acetate of lead: one equivalent of the colloid acid combines with one equivalent of lead of the neutral acetate, two equivalents of neutral lead-salt of the colloid acid being formed, $PbO \,.\, 2\,(acid) + PbO\; C_4 H_3 O_3 = 2\,(PbO \,.\, acid) + C_4 H_3 O_3$, while free acetic acid is evolved. On incinerating the acid lead-salt of the colloid acid, it chars and leaves a residue consisting of metallic lead.

By boiling the free acid with a small quantity of hydrated oxide of lead, I have often been surprised at the small proportion of lead dissolved, which is apparently owing to the lead being entirely transformed into the insoluble acid salt. After being boiled with a larger quantity of the oxide and filtered, still acid, a precipitate takes place in the fluid on cooling. This precipitate must be a lead-salt of the acid. I have not determined its quantitative composition. With an excess of hydrated oxide of lead, the colloid acid forms a compound in a great measure insoluble in hot water. After boiling the colloid acid with hydrated oxide of lead, it was observed that a yellowish-green crystalline deposit had formed in the capsule; these crystals, on being burnt, appeared to contain but traces of organic matter. I have not yet been able to determine their nature; but it is difficult to believe them to be a compound of the organic acid with lead, this acid being strictly colloid in all its other properties. When a solution of the salts of the colloid acid is boiled with hydrated oxide of lead, a portion only of the acid is precipitated; so that this method is not available for the extraction of the acid from urine.

When the colloid acid is boiled with peroxide of lead, some lead is dissolved, and the solution becomes neutral, or but very faintly acid. The acid dissolves silver from the carbonate, but it is not possible to neutralize it completely thereby. When boiled with black oxide of copper, some copper is dissolved by the colloid acid.

The Baryta-Salt.—When the colloid acid of urine is boiled with carbo-

nate of baryta, carbonic acid is evolved, the fluid becomes neutral, or very slightly alkaline, and is found to contain baryta. If the insoluble lead-salt of the acid be decomposed with sulphuric acid, and the filtrate from the sulphate of lead boiled with carbonate of baryta, the fluid becomes more decidedly alkaline. The analysis and composition of this baryta-salt has been given above; the solution in a syrupy condition deposits no crystals. The concentrated solution of the baryta-compound behaves as follows with reagents.

Basic acetate of lead:—A bulky precipitate soluble in an excess; the precipitate reappears on addition of dilute nitric acid; the further addition of nitric acid redissolves it.

Neutral acetate of lead:—A slight precipitate.

Nitrate of silver:—A slight precipitate readily soluble in nitric acid.

When the baryta-salt is boiled with carbonate of silver, but a small proportion of the metallic carbonate is decomposed even after long-continued boiling.

Acid nitrate of mercury:—A white precipitate becoming darker after a short time.

Tannic acid:—A slight precipitate.

It should be understood that the more concentrated the solution, the more abundant are the precipitates.

The Lime-Salt.—The lime-salt exhibits the same characteristic reactions as the baryta-salt; it is formed by boiling the free acid with precipitated carbonate of lime. The fluid remained acid after it had been boiled with pounded marble, although some lime was dissolved; concentrated to a certain point, the solution becomes thick and syrupy, but deposits no crystals; ammonia and oxalate of ammonia do not appear to precipitate completely the lime from the solution, but the precipitation is perfect by means of sulphuric acid and alcohol.

The Potash- and Soda-salts.—We may infer from the earthy salts that the potash- and soda-compounds of the colloid acid of urine have a slightly alkaline reaction. There is no difficulty in neutralizing a given volume of the colloid acid with a potash solution, but it is questionable whether a definite chemical compound is thus obtained.

Physiological Relations of the Colloid Acid of Urine.

I have invariably found the colloid acid present in the urine, but its mode of extraction described above is calculated to give us but a very rough insight into the quantity naturally contained in the secretion. After decomposing the lead-precipitate by sulphuretted hydrogen, a process which it must be remembered is a slow one, a given proportion of the fluid may be evaporated to dryness, the residue dried at between 101° and 110° Cent., and its weight ascertained; the result will be obtained somewhat more accurately by determining the ashes of the residue, and subtracting this weight from that of the residue. I have extracted from 8 litres of

urine 4·46 grammes of the colloid acid. This is probably hardly half the quantity contained in that bulk of the secretion. I have some reason to believe that the colloid acid described in this paper is not confined to the urinary fluid, but is found elsewhere in the human body; indeed its secretion by the kidneys shows that it very probably exists in the blood. My experiments on the blood have not yet been carried far enough to enable me to communicate the results obtained from this inquiry.

The functions of the colloid acid of urine while in the blood, assuming that it enters into its composition, must be very important. There can be little doubt that it is intimately connected with the secretion of gastric juice, by displacing the hydrochloric acid of the chloride of sodium in the blood, and transforming the soda into a colloid salt, which, from its colloid nature, would be retained in the blood, while the free hydrochloric acid would pass into the stomach to form gastric juice. I have undertaken an experiment in connexion with this point, which showed that, after dialyzing for five hours a mixture of chloride of sodium and of the colloid acid of urine, the hydrochloric acid had nearly entirely passed through the dialyzer, while rather less than half the amount of the colloid acid had remained on the diaphragm, holding some soda, though a small quantity, in solution; from an accidental omission in my notes, I regret being prevented from giving the details of the experiment.

The free colloid acid being capable to a certain extent of passing through a membrane, its secretion by the kidneys, urine being generally acid, is easily accounted for.

As to the mode of formation of the colloid acid of urine in the human body, we have, so far, no positive knowledge. From its composition and colloid nature, it may probably be derived from some transformation of the colloid non-nitrogenous product of the liver, known as the glucogenic substance.

Neubauer and Vogel's book on urine contains an account of the mode of preparation and characters of four organic acids discovered in this secretion by Städeler, these substances being phenylic, taurylic, damaluric and damolic acid. They are obtained from urine by distillation, and are crystalloids, and therefore can have no relation to the substance I have described in this paper.

When better acquainted with the chemical composition and physiological relations of the colloid acid of urine, I shall be able to give it an appropriate name.

January 12, 1865.

Major-General SABINE, President, in the Chair.

The following communications were read :—

I. "Account of Observations of Atmospheric Electricity at King's College, Windsor, Nova Scotia."—No. II. By Joseph D. Everett, M.A., F.R.S.E., Professor of Mathematics in King's College, N.S. Communicated by Professor William Thomson, F.R.S. Received December 21, 1864.

My former paper* embraced the six months from October 1862 to March 1863. Since the latter date my observations have been continued as before, the water-dropping method being employed until December 1st, since which time burning matches have been used, as in the previous winter.

The glass fibre of the station electrometer remained unchanged till July 31st, when it became loosened from its attachment, and was replaced by a new and much thinner fibre, which has continued in use ever since. From comparisons made with the portable electrometer, in the manner described in my former paper, it appears that the change of fibre has increased the indications in the ratio of 20·2 to 3·1.

The principal observations have, as before, been made three times a day, namely at 8 or 9 A.M., 2 P.M., and 9 or 10 P.M.; but additional observations have frequently been taken at other hours, especially during the months of May, June, and July, when they were much more numerous than in any month included in the former paper. Each observation has generally contained five air-readings—the interval between the readings being a minute, until September 16th, since which date it has been only half a minute. I assume that this change cannot affect the mean result, though it may to some extent influence the observed range. It was adopted for convenience, the new fibre being found to admit of more rapid observation than the old.

The following is a summary of the results of observations during rain or other downfall, fog, and thunder and lightning—the period included being the eleven months from April 1863 to February 1864.

Rain.—With light rain the electricity is generally moderate, sometimes very weak, and sometimes about double the average fair-weather strength. These remarks do not apply to light rain immediately following heavy, the electricity being often as strong during the intervals between heavy rain, and for some time after its conclusion, as during its descent. Very heavy rain is almost invariably accompanied by very strong electricity.

Snow.—Almost always positive, but occasionally a little negative intermixed with positive; and on one solitary occasion (February 16th) strong negative sparks were drawn during a heavy fall of snow. On this occasion strong positive electricity was also observed. It is worthy of remark that

* Read June 18, 1863, Proceedings, vol. xii. p. 683.

on the following morning and midday strong positive sparks were drawn, and the electricity continued very strong positive during the remainder of the day. No snow fell, but a strong west wind filled the air with drifting snow.

Hail.—I have nothing to add under this head, except that on one occasion (February 26th) strong positive sparks were drawn during hail accompanied by lightning and thunder.

Sleet.—One observation: rather strong negative.

Fog.—Always positive, and generally above the average strength, but sometimes rather below. The fogs embraced in this account were few and inconsiderable, never lasting more than a few hours, whereas the former paper included some of a more decided character.

Thunder-storms.—None of these occurred during the period embraced by the former paper; but there have been several since, and always marked by very strong electricity.

The first occurred June 15th, distant thunder commencing about 1 P.M., and a violent thunder-storm continuing from $4^h\ 30^m$ to 6^h P.M. with a deluge of rain, three-quarters of an inch falling in half an hour. Silent lightning continued all the evening, and to an unknown hour in the night. The electrometer showed, as usual, moderate positive, while the thunder was distant; but observations from $4^h\ 36^m$ to $6^h\ 2^m$ showed electricity excessively strong, with frequent changes of sign. The extremes were $+104$ and -121, the average fine-weather strength being 3 or 4.

The next storm occurred June 24th. Observations were taken from $5^h\ 11^m$ to $5^h\ 39^m$ P.M., during which time much thunder was heard, but no lightning seen. The electricity observed was constantly negative, increasing by a nearly regular advance from -29 to -214, this last being the strongest electricity that I have ever yet found. No rain fell during this observation, but ·39 of an inch fell before 9 P.M., with some heavy peals of thunder and vivid lightning. Immediately after the heaviest peal strong negative electricity was found, but was not measured.

On the evening of July 6th there was much silent lightning, the flashes being at the rate of four or five a minute, some of them very vivid. The electricity observed was weak, never rising higher than 1·8.

The next storm occurred July 18th, and closely resembled that of June 15th, but on a reduced scale as regarded its external features. The indications of the electrometer, however, were quite equal in strength to those observed on that occasion. The next day (July 19th) there was distant thunder and lightning, with what appeared to be rain in the distance, from about 3 to 4 P.M.; and the electricity observed was very strong negative, observations extending from $3^h\ 11^m$ to $3^h\ 57^m$. The observations on these two days are given *in extenso* at the end of this paper.

Silent lightning was observed on the evening of August 6th, the electricity indicated being moderate positive.

On August 10th there was a deluge of rain with some thunder and light-

ning, during which frequent observations were taken, showing very strong electricity, generally negative.

No more instances occurred till the evening of February 26th, when hail fell, with short intermissions, from a little before $9^h\ 30^m$ P.M. till after midnight, accompanied by much lightning and some thunder. The only observation of the electrometer was at $9^h\ 30^m$, when strong positive sparks were obtained.

It appears from these instances, that thunder-storms in the neighbourhood of the place of observation are accompanied by extremely strong indications of atmospheric electricity, but that neither silent lightning nor the distant rumbling of thunder is accompanied by any marked effect on the electrometer.

For the sake of comparison with numerical data given in the former paper, applying to the six months October 1862 to March 1863, I subjoin the corresponding data for April to September 1863, thus completing a year from the commencement of observations.

	Positive only.	Negative only.	Both kinds.
	Days.	Days.	Days.
Rain	17	7	12
Snow	1	0	2
Hail	1	1	0
Fog	3	0	0
Thunder or lightning	2	2	3

There were 34 days on which both positive and negative electricity were observed; and on 29 of these, rain or other downfall occurred. The remaining 5 days, with the strongest negative observed, and the state of cloud and wind at the time, were as under (the scale for cloud being 0–10, and for wind 0–6).

May 31.	−0·4	10 nim.	1 S.W.
June 12.	−0·4	10 all sorts.	1 N.E.
July 6.	−0·1	7 {nim. intensely black.	4 {N.W. a brief squall.
July 15.	−0·8	10 st.	1 N.
Aug. 24.	−1·0	9 cu.-st.	1 S.W.

It will be observed that in all these instances the weather was cloudy and the negative electricity weak, characteristics which also belong to the corresponding instances in the former paper. The remark there made, that on every day on which negative electricity had been observed, positive had also been observed, holds good down to the present date (March 1864).

The monthly means of the results of fine-weather observations, for different hours of the day, from April to September 1863, are shown in the following Table. They have been computed in the same manner as the corresponding numbers in the former paper.

Hour.	April.		May.		June.		July.		August.		September.	
	Nr. of observations.	Mean.	Nr. of observations.	Mean.	Nr. of observations.	Mean.	Nr. of observations.	Mean.	Nr. of observations.	Mean.	Nr. of observations.	Mean.
6 to 7 A.M.	...	......	...	...	1	2·20	2	3·80	1	3·39		
7 to 8	5	4·82	2	3·40	8	3·28	7	4·16	5	4·40	12	3·85
8 to 9	16	4·46	19	3·15	16	2·94	18	3·06	12	3·90	7	3·63
9 to 10	3	4·37	11	2·80	8	2·95	13	3·34	4	4·54	4	4·75
10 to 11	2	5·70	3	2·27	4	2·60	12	2·88	7	4·10	1	5·00
11 to 12	4	3·10	2	2·95	2	2·10	10	2·71	2	2·33		
12 to 1 P.M.	4	4·15	3	3·30	4	2·60	8	3·32	5	4·52	1	4·96
1 to 2	9	4·31	3	3·57	9	3·29	19	3·10	10	3·80	8	4·75
2 to 3	14	4·92	19	3·47	15	2·88	10	3·12	7	3·99	8	4·13
3 to 4	...	...	3	3·93	6	2·94	10	3·23	2	4·51	2	2·78
4 to 5	6	3·77	7	3·50	6	3·17	9	3·60	2	3·35	1	7·62
5 to 6	5	3·84	8	3·69	5	3·22	9	2·92	2	4·05	4	4·15
6 to 7	4	4·02	5	3·48	5	3·02	9	3·86	6	3·27		
7 to 8	3	3·50	3	3·83	3	3·10	6	2·33	6	4·75		
8 to 9	...	...	1	1·50	1	3·80	3	3·33	3	3·00	3	3·25
9 to 10	12	3·18	22	2·97	21	2·89	14	1·93	8	2·60	11	3·39
10 to 11	4	2·80	3	2·17	4	2·40	12	2·01	4	3·29	3	2·46
11 to 12 P.M.	2	3·35	6	1·70	3	2·97	1	2·90	3	1·96	1	2·80
12 to 2 A.M.	1	2·20	1	4·60	4	1·68	...	...	2	2·77		
Sums and means. } ...	94	4·08	121	3·13	125	2·83	172	3·01	91	3·71	66	3·92

For the whole six months we have the following results:—

Hour.	Number of observations.	Mean of all observations.	Mean of monthly means.
6 to 7 A.M.	4	3·30	3·13
7 to 8	39	3·96	3·98
8 to 9	88	3·47	3·52
9 to 10	43	3·44	3·79
10 to 11	29	3·33	3·76
11 to 12	20	2·71	2·64
12 to 1 P.M.	25	3·64	3·81
1 to 2	58	3·69	3·80
2 to 3	73	3·70	3·75
3 to 4	23	3·33	3·48
4 to 5	31	3·64	4·17
5 to 6	33	3·51	3·64
6 to 7	29	3·55	3·53
7 to 8	21	3·51	3·50
8 to 9	11	3·11	2·98
9 to 10	88	2·71	2·74
10 to 11	30	2·40	2·52
11 to 12	16	2·33	2·61
12 to 2 A.M.	8	2·38	2·81
Means of columns } ...	...	3·45	3·38

Hence there appears to be a maximum soon after sunrise, a decided minimum between 11 and 12, and a maximum (less clearly marked) between 4 and 5 P.M., followed by a regular decrease to midnight. These

results agree very well with those derived from the previous six months, allowing for the difference between the length of the day in summer and in winter.

The following Table of the variations of electricity in fine weather, from month to month, embraces the whole period of observation down to February 1864. These results, as well as those above given, are expressed in units of station electrometer with second fibre, being the same unit that was employed in the previous paper.

The day is supposed to be divided into three portions—before noon, noon to 6 P.M., and after 6 P.M. For each month, all the observations in each portion have been summed and divided by their number, giving the means shown below.

Year.	Month.	Before noon.	Noon to 6 P.M.	After 6 P.M.	Mean of three preceding columns.
1862.	October.	3·42	3·68	2·69	3·26
	November.	3·53	2·89	2·58	3·00
	a December.	4·09	5·01	2·77	3·96
1863.	January.	4·11	4·88	3·42	4·14
	February.	6·10	5·77	4·96	5·61
	March.	6·28	5·10	5·02	5·47
	April.	4·41	4·37	3·26	4·01
	May.	2·98	3·54	2·85	3·12
	June.	2·91	3·02	2·52	2·82
	b July.	3·17	3·20	2·50	2·96
	August.	3·98	4·01	3·20	3·73
	September.	3·98	4·41	3·18	3·86
	October.	5·24	4·16	2·74	4·05
	November.	4·24	4·13	2·82	3·72
	December.	4·51	5·14	3·39	4·35
1864.	January.	3·86	5·74	3·63	4·41
	c February.	4·78	4·97	3·16	4·30

a. Second fibre put in December 6th.

b. Third fibre put in July 31st.

c. The electricity on February 17th and part of 18th was out of range, and has not been reckoned.

These results show that atmospheric electricity is stronger in winter than in summer, and seem to indicate a double maximum and minimum within the year,—the principal maximum occurring about February, and the other maximum about October; the principal minimum in June, and the other in November. It will be observed that in every case the numbers in the column "after 6 P.M." are the smallest.

At the suggestion of Professor Thomson, I have made a careful comparison of the states of electricity, as regards both strength and variableness, for different directions of wind. For this purpose I have tabulated according to direction of wind (separating also fine-weather from wet-weather observations) the daily entries of mean potential at 2 P.M. for the first twelve months, also the variableness as measured by the difference between the entries of highest and lowest potential for the same hour. Where there was no observation between 3 and 4 P.M. the day was passed over;

and where more than one observation was entered between these hours, that which was nearest to 2 P.M. was alone reckoned.

From these data, the monthly means of strength and variableness were computed; but in neither case was any regularity exhibited. The only results of this comparison which seem worthy of record are the annual fine-weather means (derived from the monthly) for the prevailing directions of wind. These are—

	Calm.	S.W.	N.	N.W.	W.
Strength	4·29	3·63	4·03	4·48	4·05
Variableness ..	1·19	·88	1·22	1·71	·79

I append, by way of specimen, the observations taken on June 29, July 1, 18, and 19. The first two days contain instances of some of the weakest electricity that I have ever found in clear weather (I allude particularly to the observations at 2^h 10^m June 29th, and at 3^h 47^m July 1st). The other two days afford fair instances of observations during thunder and lightning*.

	Electricity.			Rain.	Cloud.		Wind.		Barometer uncorrected.	Thermometers.	
	Mean.	Highest.	Lowest.		0–6.		0–10.			Dry bulb.	Dry above wet.
June 29.											
h m										°	°
8 48 A.M.	+ 2·1	+ 2·2	+ 2·1	....	0	......	0	Calm.	30·05	67·8	3·8
2 10 P.M.	+ 0·5	+ 0·8	+ 0·2	....	2	Cu.	2	N.	30·03	78·1	7·8
9 27	+ 1·8	+ 1·8	+ 1·7	....	0	......	0	Calm.	30·07	69·4	3·6
July 1.											
7 31 A.M.	+ 2·3	+ 2·3	+ 2·2	....	0	......	0	Calm.	30·12	62·5	2·5
8 20	+ 1·8	+ 1·9	+ 1·7	....	0	......	0	"	30·12	65·9	4·0
9 49	+ 1·8	+ 1·9	+ 1·6	....	0	......	0	"	30·11	70·4	7·1
3 47 P.M.	+ 0·8	+ 1·0	+ 0·5	....	0	......	1	S.W.	30·08	81·3	12·1
8 58	+ 1·6	+ 1·7	+ 1·4	....	1	St.	0	Calm.	30·12	66·6	5·3
10 29*a*	+ 1·6	+ 1·6	+ 1·5	....	0	......	0	"	30·12	60·6	3·3
July 18.											
8 16 A.M.	+ 2·2	+ 2·3	+ 2·1	....	1	Cu.	0	Calm.	30·28	68·2	1·9
11 58	+ 2·9	+ 3·0	+ 2·9	....	5	Cu. & st.	0	"	30·28	77·5	4·3
1 51 P.M.	+ 3·6	+ 3·8	+ 3·4	....	2	Cu.	1	N.W.	30·27	79·3	5·8
3 21*b*	− 24·3	−20·2	−27·6	....	9	Nim.&cu.	1	N.W.	30·26	77·4	4·3
3 40	− 30·1	−26·9	−32·4	Moderate rain ...	9	"	...	......	30·26	77·4	4·0
3 49	− 59·6	−43·9	−76·9	Pouring rain ...	9	Nim.	1	N.W.	30·27	76·0	3·2
3 59	− 90·2	....		Light rain.							
4 0	−105·4										
4 1	− 80·3										
4 2	− 93·6										
4 3	− 86·8	Peal overhead.									
4 3½	− 49·4										
4 4	− 64·3										
4 6	− 74·2										
4 7	− 55·9	[head.		Rain heavy.							
4 8	− 1·2	Rumbling over-		[rate.							
4 9	− 19·5			Rain mode-							
4 10	+ 24·1										

a. Aurora.

b. Continuous rumbling of distant thunder in N.W., lasting till 5 P.M.

* The details of daily observations from April 1, 1863, to February 28, 1864, inclusive, are given in a series of Tables which are preserved for reference in the Archives.

TABLE (*continued*).

	Electricity.			Rain.	Cloud.		Wind.		Barometer uncorrected.	Thermometers.	
	Mean.	Highest.	Lowest.		0–6.		0–10.			Dry bulb.	Dry above wet.
July 18.											
h m											
4 10½ P.M.	− 18·5	Flash.									
4 11	− 45·7	Heavy peal.									
4 12	−111·0			[rate.							
4 12½	− 81·9	....		Rain mode-							
4 14	−146·0	Flash.									
4 15	−138·0										
4 16	−120·0		[der.								
4 17	−107·0	Heavy thun-									
4 17½ *a*	−129·0	....		Heavy rain.							
4 18	− 61·8	....		Pouring							
4 19	− 74·2	....		" [rain.							
4 19½	− 80·0	....		"							
4 20	− 62·7										
4 24	− 73·5										
4 25	+ 2·2										
4 33	− 53·2			[rain.							
4 34	− 65·2	....		Pouring							
4 35	− 3·7	....		"							
4 37	− 27·8		[tinuous.								
4 38	+ 4·6	Thunder con-									
4 39½	− 16·1										
4 40	− 80·7										
4 41	−101·0	[overhead.									
4 41½	+ 73·2	Heavy clap									
4 42	+ 46·4			[rate.							
4 43	+ 38·0	....		Rain mode-							
4 44	+ 14·8	....		Rain light.							
4 45	− 2·2										
5 8	+ 24·7	+25·9	+22·8	......	...	......	...	......	30·28	74°·7	2°·8
6 7	+ 5·7	+ 5·8	+ 5·5	......	6	Cu. &c.	0	Calm.	30·29	75·9	2·7
8 8	+ 3·8	+ 3·8	+ 3·8	......	7	Cu.-st.&c.	0	"	30·30	75·4	2·7
10 0	+ 1·8	+ 2·0	+ 1·5	......	9	Nim.	1	N.W.	30·30	69·2	1·3
The weather has been extremely oppressive all day. Amount of rain ·31 inch.											
July 19.											
9 49 A.M.	+ 6·1	+ 7·2	+ 5·0	......	10	Nim.	1	N.	30·29	70·6	2·0
1 3*b* P.M.	+ 3·1	+ 3·5	+ 2·9	......	3	Cu. & ci.	0	Calm.	30·23	76·3	3·5
Thunder and lightning in W. and N.W. Apparently raining there.											
3 11 P.M.	− 34·9	....		....	9	Nim.&cu.	...	......	30·20	74·2	2·9
3 12	− 38·3										
3 13	− 37·1										
3 16	− 20·7	....		Raining.				[ing in N.W.			
3 22	− 23·5	....		Not raining here, but thundering and apparently rain-							
3 32	− 17·0	Thundering in N.W.									
3 35	− 16·7	Flash in N.W.									
3 57	− 20·1	....		....	....	....	....		30·20	75·6	3·0
4 12	− 9·9	Thunder in N.E.		......	8	Nim.&cu.	2	N.			
9 18	+ 2·4	+ 2·8	+ 1·8	......	10	Nim. &c.	0	Calm.	30·20	68·6	1·8

a. Continuous rumbling of distant thunder till 5 P.M., followed by occasional distant thunder till about 5^h 15^m P.M.

b. Excessively close.

II. "Notes of Researches on the Acids of the Lactic Series.—No. II. Action of Zinc upon a Mixture of Iodide of Ethyl and Oxalate of Methyl." By EDWARD FRANKLAND, F.R.S., and B. F. DUPPA, Esq. Received December 20, 1864.

In our former communication * on the action of zinc upon a mixture of iodide and oxalate of methyl, we described a process by which the use of the zinc-compounds of the alcohol radicals may be dispensed with in the production of the series of acids which we are now investigating. We then described this process as being conducted at a temperature of 70° to 100° Cent. for twenty-four hours, until the mixture had solidified to a yellowish gum-like mass, which on distillation yielded a mixture of water, alcohol, and the ether of the new acid. Subsequently we have found it more advantageous to continue the operation for a much longer time at a lower temperature, thereby obtaining a crystalline instead of a gum-like product, the former giving a much better result as regards the production of ether.

In the reaction which forms the subject of the present Note, we have proceeded in the following manner. Two atoms of iodide of ethyl were mixed with one of oxalate of methyl and placed in a capacious flask, with zinc in sufficient quantity to be barely covered by the ethereal mixture. We prefer to use zinc which has been employed in a previous operation, as we find it to act not only with greater rapidity, but also at a much lower temperature. The time required for the completion of an operation is about ninety-six hours at a temperature of from 30° to 50° Cent. During the first eighteen or twenty hours no apparent action takes place, the liquid remaining perfectly limpid, and the zinc apparently untouched; but after this period a straw-coloured tint gradually makes its appearance and slowly increases in intensity, until the liquid solidifies to a mass of crystals which scarcely fuse at 50° Cent. The operation may now be considered as ended, although a considerable quantity of the mixed ethers is still unacted upon. Water being now added by slow degrees until it equals three times the volume of the crystalline mass, a copious effervescence takes place; oxalate and oxide of zinc are formed in abundance, whilst, on the application of heat, alcohol, accompanied by a considerable quantity of an ethereal body, distils over along with the iodide of ethyl that has not been acted upon. The addition of water to the distillate effects an approximate separation of the ethereal from the alcoholic portion; the former is then decanted and distilled for the purpose of separating alcohol and iodide of ethyl. When the temperature of ebullition rises to 100° Cent., the liquid left in the retort is placed over chloride of calcium for twelve hours, after which it is again submitted to distillation, when its boiling-point almost immediately rises to 165° Cent.

* Proc. Roy. Soc. vol. xiii. p. 140.

(bar. 29·85 in.), at which temperature the whole of the remaining liquid passes over. Submitted to analysis, this liquid yielded results closely corresponding to the formula

$$C_7 H_{14} O_3.$$

The decomposition of this ether by baryta, described below, proves it to be the methylic ether of an acid of the same composition as leucic acid, with which also it agrees in its fusing-point. The composition of this ether may therefore be thus expressed:—

$$\left\{\begin{array}{l} C^{iv}\left\{\begin{array}{l}C_2H_5\\ C_2H_5\\ OH\end{array}\right. \\ C^{iv}\left\{\begin{array}{l}O\\ OCH_3\end{array}\right.\end{array}\right.$$

Leucate of methyl is a colourless, transparent, and tolerably mobile liquid, possessing a peculiar ethereal odour only remotely resembling leucate of ethyl. It is very sparingly soluble in water, but readily soluble in alcohol or ether. Its specific gravity is ·9896 at 16°·5 C.; it boils at 165° and distils unchanged. A determination of its vapour-density gave the number 4·84, the above formula corresponding to two volumes of vapour ($H_2O=2$ vols.) requires the number 5·03.

Treated with caustic alkaline bases this ether is readily decomposed, even in the cold, yielding methylic alcohol and a leucate of the base. A quantity of it was thus decomposed with solution of baryta, the excess of the base being afterwards removed. It yielded on evaporation a crystalline mass very soluble in water, alcohol, and ether, and which, on analysis, yielded results closely corresponding with those calculated from the formula of leucate of baryta.

$$C_2^{iv}\left\{\begin{array}{l}(C_2H_5)_2\\ (C_2H_5)_2\\ (O\,H)_2\end{array}\right.$$

$$C_2^{iv}\left\{\begin{array}{l}O_2\\ \left.\begin{array}{l}O\\ O\end{array}\right\}Ba\end{array}\right.$$

When this baryta-salt in aqueous solution is decomposed with the exact amount of sulphuric acid necessary, the liquid filtered off from the sulphate of baryta, and evaporated *in vacuo*, the acid crystallizes magnificently. Professor W. H. Miller has kindly undertaken the determination of the angles of these crystals. They are readily soluble in ether, alcohol, and water. The acid is greasy to the touch, and nearly inodorous. It sublimes readily at 50° C., and slowly even at common temperatures, a small quantity of the acid left on a watch-glass gradually disappearing, though in other respects it is permanent when exposed to the air. It fuses

at 74°·5 C. Numerous analyses furnished the numbers required by the formula

$$\left\{\begin{array}{l} C^{iv}\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ OH \end{array}\right. \\ C^{iv}\left\{\begin{array}{l} O \\ OH \end{array}\right. \end{array}\right.$$

Leucate of silver was made by adding oxide of silver to a hot solution of the acid. After filtration and evaporation *in vacuo,* it crystallizes in brilliant silky fibres adhering closely to the capsule. These are anhydrous, and are scarcely discoloured by prolonged exposure to a temperature of 100° C. They yielded on analysis numbers closely corresponding with those calculated from the formula

$$\left\{\begin{array}{l} C^{iv}\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ OH \end{array}\right. \\ C^{iv}\left\{\begin{array}{l} O \\ O\,Ag \end{array}\right. \end{array}\right.$$

Although this acid possesses the same percentage composition, atomic weight, and fusing-point as the leucic acid obtained by the action of zincethyl upon oxalic ether, yet it does not appear to be identical with that acid. The silver-salt of the latter crystallizes in brilliant needles radiating from centres standing up freely from the capsule, and containing half an atom of water which is not expelled at 100° C. This salt also further differs from that above described by becoming rapidly discoloured when exposed to the heat of a steam-bath. We are at present engaged with a rigorous comparison of the properties of these and other similarly related acids of the lactic series.

III. "Preliminary Note on some Aluminium Compounds." By George Bowdler Buckton, F.R.S., and William Odling, M.B., F.R.S. Received January 12, 1865.

Until recently the molecule of aluminic chloride had always been represented by the formula $Al_2\,Cl_3$, or, selecting the high atomic weight of aluminium, as required by its specific heat, $Al\,Cl_3$. But since Deville's determination of the vapour-densities of aluminic and ferric chlorides, many chemists of eminence, both in this country and abroad, have adopted the formula $Al_2\,Cl_6$, and have consistently doubled the previously received formulæ for the entire series of aluminic compounds. In our opinion, however, the hitherto existing data seemed hardly sufficient for the definitive establishment of either set of formulæ; and it occurred to us that an examination of the so-called organo-compounds of aluminium might not improbably throw some important light upon the question at issue between them. We regarded the determination of the question as a matter of con-

siderable interest from the bearing it would necessarily have upon the position of aluminium in a natural classification of the elements; upon the molecular formulæ of chromic, ferric, cuprous, and perhaps mercurous compounds; and consequently upon Laurent and Gerhardt's general law of even numbers. Moreover a satisfactory investigation of the organo-compounds of a metal certainly not belonging to any one of the recognized classes of metals with whose organo-compounds chemists have become familiar, seemed likely to furnish a useful contribution to the common knowledge of organo.metallic bodies. Cahours, in an admirable paper on the organo-compounds of tin, published early in 1860, observed incidentally that aluminium was attacked by the iodides of methyl and ethyl at the temperatures 100°–130°, and that the crude ethylated product reacted violently with zinc-ethide to form a very inflammable liquid, which was doubtless aluminium ethide. Our experiments in confirmation of Cahours's results have been as yet merely preliminary; but by acting on aluminium with mercuric methide and ethide at the temperature of 100°, we have obtained pure aluminium methide and ethide without difficulty, and in not inconsiderable quantity. This mode of experiment was obviously suggested by Frankland and Duppa's recently described processes for making methide and ethide, and for transforming these compounds into zinc methide and zinc ethide respectively.

Aluminium Ethide.

Mercuric ethide with excess of aluminium-clippings contained in sealed tubes was heated for some hours in a water-bath, when the mercury was found completely displaced by the aluminium, thus,

$$Al_2+3\,Hg\,Et_2=Hg_3+2Al\,Et_3,\text{ or }Al_2\,Et_6.$$

After distillation off fresh aluminium and rectification in an atmosphere of hydrogen, the resulting aluminium ethide boiled steadily at 194°. It occurred as a colourless mobile liquid, which did not solidify at the temperature of −18° C. It evolved dense white fumes on exposure to air, and when in thin layers took fire spontaneously, burning with a bluish red-edged flame, and producing an abundant smoke of alumina. On analysis it yielded 61·4 per cent. of carbon, 12·9 per cent. of hydrogen, and 24·0 per cent. of aluminium, numbers which accord reasonably well with the formula $Al\,Et_3$, or $Al_2\,Et_6$, the carbon and hydrogen being slightly deficient from some unavoidable oxidation of the substance analyzed. Its vapour-density, taken by Gay-Lussac's process at the temperature 234°, was found to be 4·5, the theoretical density calculated for the formula $Al\,Et_3$ being 3·9, and that for the formula $Al_2\,Et_6$ being of course 7·8. Hence aluminium ethide would appear to have the simple molecular formula $Al\,Et_3$; for the difference between the experimental number 4·5 and the theoretical number 3·9, is an obviously necessary consequence of the extreme oxidizability of the compound. Water effected a complete decomposition of aluminium ethide with explosive violence. Iodine reacted upon it, to produce iodo-derivatives and iodide of ethyl. Oxygen in the form of dry air was simply absorbed with production of a body apparently analogous to boric

di-oxyethide. But the iodo-derivatives and oxidation products have as yet been submitted to a preliminary examination only.

Aluminium Methide.

This compound was obtained by a process strictly analogous to that which yielded us aluminium ethide. On heating mercuric methide with aluminium-clippings in a water-bath, the replacement of the mercury by aluminium took place with even greater facility than was manifested during the similar treatment of the ethylated body. After a single distillation, aluminium methide occurred as a colourless mobile liquid, boiling steadily at 130°, and solidifying a few degrees above 0° into a beautiful transparent crystalline mass. The liquid took fire spontaneously on exposure to air, burning with a very smoky flame, and producing abundant flocculi of alumina discoloured by soot. On analysis, aluminium methide gave 48·4 per cent. of carbon, 12·3 per cent. hydrogen, and 38·2 per cent. aluminium, numbers which are quite sufficiently in accordance with the formula $Al\,Me_3$, or $Al_2\,Me_6$. Three separate determinations of vapour-density, made at the temperatures of 240°, 220°, and 220°, the last with hydrogen in the tube, gave the numbers 2·80, 2·80, and 2·81 respectively, which agree closely with the theoretical number calculated for the formula $Al\,Me_3$, namely 2·5. But the corrected density increased very rapidly with every decrease of temperature, a peculiarity of behaviour also noticed by Frankland in the case of boric methide. Thus three separate determinations, made at 163°, 160°, and 162°, the last with hydrogen in the tube, gave the densities 4·1, 4·1, and 3·9 respectively; while the determinations made at the boiling-point of aluminium methide, of course with hydrogen in the tube, as recommended by Playfair and Wanklyn, gave the densities 4·36 and 4·40 respectively, which approximate somewhat to the theoretical density 5·0, calculated for the formula $Al_2\,Me_6$. Hence aluminium methide appears to be a member of that class of bodies whose vapour-densities are under certain circumstances anomalous, either because the bodies exist in two molecular states of condensation, or because their vapours are not possessed of perfect elasticity until heated considerably above the boiling-points of the respective liquids. In either case the question naturally presents itself, May not the only observed vapour-density of aluminic chloride correspond to the high vapour-density of aluminium methide, and may they not both be equally anomalous, and consequently untrustworthy as a basis for determining the general formulæ of aluminic compounds?

January 19, 1865.

Sir HENRY HOLLAND, Bart., Vice-President, in the Chair.

The following communications were read:—

I. "On Bubbles." By Frederick Guthrie, Esq., Professor of Chemistry and Physics at the Royal College, Mauritius. Communicated by Professor Stokes, Sec. R.S. Received December 22, 1864.

As it was found necessary, in considering drops*, to define the term, and limit its application, so we must understand once for all in what sense and under what restrictions the term bubble is to be employed. This is the more necessary, because the word bubble is used even more loosely than the word drop. In Plate I. fig. A, 1, 2, and 3 show the meaning of a drop as we have defined and used the expression; 4 shows the condition of a bubble as it is understood in the following investigation.

Under this limitation, a bubble XGL† only differs from a drop XL_2 L_1 (3, fig. A) in consisting of a gas instead of a liquid. A bubble is a mass of gaseous matter compelled to assume a more or less spherical form by the cohesion and weight of the liquid medium in which it is formed, and separated from other matter by the action of gravity. Since, under like conditions of pressure, all gases are lighter than all liquids, the separating force is the gravity of the medium, as was the case with the drop (3, fig. A). Accordingly, a bubble invariably ascends. Owing to the universal diffusion of gases, no case can exist of a gas-bubble in a gaseous medium (XGG); and for obvious reasons a solid medium is inadmissible. So defined, a bubble must therefore invariably be a case of XGL.

It is, however, worth while, in passing, to notice the construction of some other bodies which are also called drops and bubbles. Thus all the states of matter shown in fig. B are called, in common speech, drops or bubbles; and some of them, indeed, are one or the other, according to the aspect in which they are viewed. All of the ten modifications in fig. B are very common: the Nos. 1, 2, 3, 4, 5, 6 are usually called drops; the Nos. 7, 8, 9, 10 are called bubbles. Nos. 4 and 5 show the two instances of what is called spheroidal state. Nos. 7, 8, 9, 10 are the commonest forms of the soap-bubble. The equations under each figure show the possible identity of two matters of the same kind. All the above ten cases are at once distinguishable from the true drop and bubble by the existence in them of an additional factor, which is not present in the true drop or bubble, namely the cohesion of a *film*. Such drops and bubbles may therefore be conveniently distinguished from the true ones of fig. A by being called *film-drops* and *film-bubbles*. In the spurious drops 1, 2, 3, 5, the film partly enclosing and restraining the drop is a film of liquid: so also in the bubble

* See the author's Memoir on Drops, Proceedings, vol. xiii. p. 444.

† Where X is either solid, liquid, or gaseous.

Fig. A

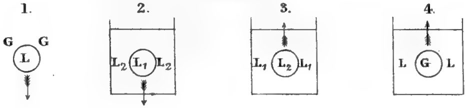


Fig. B.

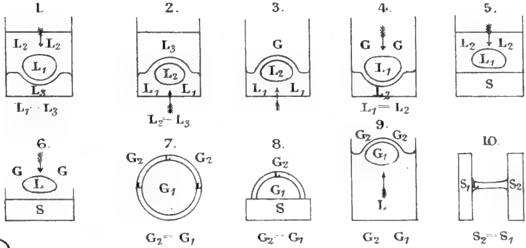


Fig. C.

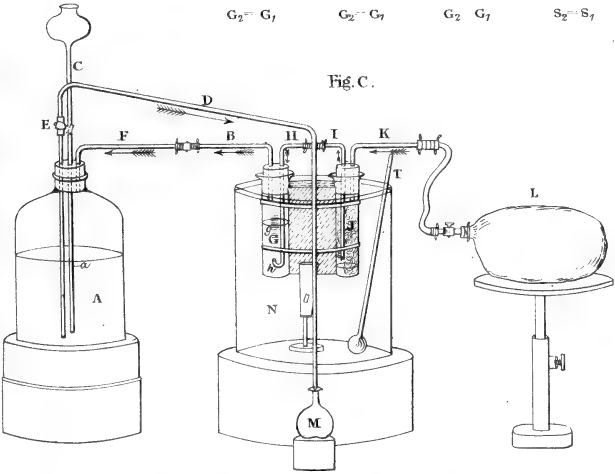
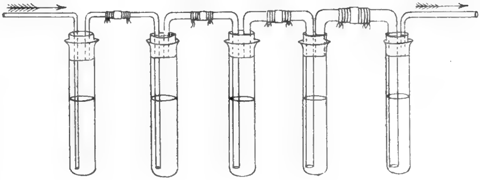


Fig. D.



J. Basire, lith.

9. In the drops 4, 6, and in the bubbles 7, 8, the restraining film is a gas. There is a remarkable inverse analogy between the cases 4 and 9. In 4 a gaseous film hinders a liquid from reaching a liquid; in 9 a liquid film hinders a gas from reaching a gas. The cases 7 and 10 are also called bubbles, although their only title to the name is the liquid film in each. Viewed as film-envelopes, 1, 2, 3, 4, 5, 6 are bubbles; viewed as spheroidal liquid masses they are drops.

Further, the spurious drops and bubbles differ from the true ones which we have to examine in a very important particular. The spurious ones are essentially statical phenomena, and retain their indefinite size for an indefinite time. The true drops and bubbles, on the other hand, grow until their exact equilibrium is established, and they acquire their definite size at the instant of the overbalancing of that equilibrium—that is, at the instant of their motion. It is, in fact, this overbalancing which determines the definiteness of their size, by withdrawing them from the size-determining effect of the action of the contending forces which accompany and condition their growth.

All attempts to get a perfectly uniform succession of bubbles of the pure form SGL (corresponding to water dropping from a glass sphere) failed through the impossibility of getting the immersed solid protected by the gas from the adhesion of the liquid. But by a contrivance similar to that described in the case SL_1L_2, where L_1 was lighter than L_2*, it was found possible to get bubbles of uniform size, and to measure them.

The most obvious manner of doing this is to force a gas at a fixed rate through an ordinary gas-delivery tube, and to collect and measure a given number of the bubbles in a calibrated tube over the pneumatic trough. This plan, however, is open to the objection of requiring a large quantity of liquid medium.

The apparatus employed is seen in fig. C. The quart bottle A is filled a little above the mark *a* with water, which is in some experiments covered with a film of oil. Through its cork three tubes, C, D, F, pass absolutely air-tight. The tube C is a simple funnel-tube, open near the bottom of A. The tube D also reaches to the bottom of A, and acts as a siphon: its longer limb is narrowed at the point, and delivers its water into the little flask M, whose neck bears a mark *m*. The shorter limb of D bears a cock E to regulate its discharge. The third tube F, which opens immediately under the cork of A, is fastened by a caoutchouc joint to the tube B. In this joint, and pressing the ends of both tubes, is a compact mass of cotton-wool. B passes through the cork of the little test-tube G, which is divided into millimetres, and contains the liquid through which the bubbles are to pass. Through the cork of G another tube H is passed, whose lower end *h* is bent out horizontally, and is beneath the surface of the liquid in G; H is connected by a caoutchouc joint with I, which passes nearly to the bottom of a second little test-tube J. The tube J contains a few drops of the liquid which is in G, and the space between I and

* On Drops, p. 478.

the sides of J is filled with cotton-wool moistened with the same liquid. The last tube K, which opens immediately under the cork of J, is either open to the air, or connected with a gas-bag containing the gas under examination, or fastened to a chloride-of-calcium tube, according to the requirements of the experiment. In some experiments the little tubes G and J are surrounded with water contained in the vessel N. The tubes G and J are firmly bound to a flat piece of cork held by the heavy clamp P, which rests on the bottom of N. A thermometer T is placed in the water of N.

The apparatus is used as follows:—B and F being disconnected, the bottle A is nearly filled through C. The end of F is closed by the finger, and, the stopcock E being opened, the siphon D is filled once for all by applying the mouth to its longer end. E being then closed, the tube G is filled up to the required mark with the liquid which is to serve as a bubble-medium. The cotton-wool in J is moistened with the same liquid. All the joints are made fast, and the tube K is connected with the gas-bag L. On turning the stopcock E, water flows through the siphon D into the flask M: to supply its place, gas must enter by F; that is, gas must bubble through the liquid in G. Before entering G it becomes saturated with the vapour of the same liquid in J. If all the joints are tight, it follows that the volume of water entering M is equal to the volume of gas which bubbles through the liquid in G. It is a sufficient test of the tightness of all the joints (as far as H), to run off a little water by D, so as to bring a bubble or two of gas through *h*, and to allow the apparatus to rest. If the tube H remains full of air to its extremity for a quarter of an hour, the apparatus may be considered as air-tight. A metronome is adjusted to beat to the required time. M is removed and emptied. E is turned till the bubbles, passing through the liquid in G, are synchronous with the beats of the metronome. This rate is maintained until the liquid in A sinks to *a*. The flask M is then put in its place, and from that instant the bubbles through G are counted. When M is filled exactly up to *m*, the experiment is finished. The proximity between M and G enables the eye to count the bubbles, and to watch without difficulty, at the same time, the rise of the liquid in M. The contents of M, divided by the number of bubbles, gives the mean volume of a single bubble. The use of the cotton-wool in the joint between B and F is to check the flow of gas through the apparatus. When this plug is absent, the considerable volume of gas in the upper part of A, being in direct communication with G, causes by its elasticity an irregular delivery of bubbles through G. Of course as M is filled the level of the liquid in A falls, the difference between the limbs of the siphon D is diminished, the flow through D is retarded, and the bubbles follow one another more slowly. We shall see, however, that the rate of sequence has exceedingly small, or absolutely no influence upon bubble-size. In the experiments actually performed to establish this fact, the metronome was allowed to continue beating throughout the experiment, and an occasional tap on the cock E was found sufficient to regulate the rate of sequence with perfect accuracy. The great comparative

volume of A, moreover, prevents the level of its water from undergoing more than a very slight variation during a single experiment.

Certain modifications were introduced in the apparatus for special purposes, which will be described in their proper places.

Judging by analogy from the results obtained with drops, we should conclude the bubble-size to be influenced mainly by

1. Rate of sequence, or value of gt.
2. Chemical nature of bubble-gas, if homogeneous. Proportion between its constituents, if heterogeneous.
3. Nature of solid from which the gas is delivered.
4. Size of orifice and geometric distribution of solid about its orifice.
5. Temperature of gas and medium.
6. Tension of gas, influenced by natural or artificial causes.
7. Chemical nature of liquid, if homogeneous; and proportion between its constituents, if heterogeneous.

As in the cases of SLG and SLL, the solid serves mainly as a support to the dropping liquid, and influences the size of the drop by the various ways in which it affects the liquid film which adheres to the solid; the actual disruption being between liquid and liquid: so in SGL the bubble parts in truth from gas.

The separation of a gas-bubble differs materially from that of a drop in this respect. In the case SLG it is the persistent cohesion of the liquid which gives the drop a spheroidal form, and thereby assists gravitation to overcome the stubborn cohesion of the liquid. In the case of SLL the separation is assisted by the persistent cohesion of the liquid medium, which also tends to mould the drop into a spherical form, and is hindered by the stubborn cohesion and weight of the medium, which, by resisting its descent, increases its weight. In the case of a bubble, the ascent of the bubble is due wholly to the descent of the liquid medium; and the spheroidal form of the bubble is due wholly to the persistent cohesion of the liquid medium; for this cohesion is most completely satisfied when the cavity containing the gas is most spherical.

We may now examine *seriatim* the influences of the seven conditions noticed above.

SGL.

Influence of rate.—To examine the effect of variation in gt we may take common air as the gas, distilled water as the liquid medium, the tube H of glass having an opening h of any convenient unmeasured size; and as we are not concerned with the absolute, but with the relative sizes of the bubbles, the vessel M may be of an indefinite size.

Table α shows the effect of variation in gt alone. Column 1 shows the values of gt. Column 2 the sequence of the experiments. Column 3 the number of bubbles. Column 4 the mean relative size of a single bubble at the respective rates of column 1.

TABLE α.

From glass, air-bubbles through water.

T=23° C.

B=767 millims.

1. *gt.*	2. Sequence of experiment.	3. Number of bubbles having together the volume M.	4. Relative mean volume of single bubble.
0·33	7	96	96·66
	8	97	
	9	97	
0·50	4	100	99·80
	5	99	
	6	100	
	16	100	
	17	100	
1·00	3	104	103·00
	10	103	
	11	103	
	19	103	
	2	103	
	14	102	
2·00	12	97	99·17
	13	98	
	15	98	
	18	98	
	20	103	
	21	101	
5·00	1	103	103·00

It would at first seem as though there were a well-marked difference depending upon the value of gt. But in this method of experimenting there is a possible maximum error of two bubbles in each case, or an error of four bubbles in the comparison of two instances. This nearly covers the observed discrepancy. To set this point at rest, experiments were made with a larger number of bubbles as follows. The vessel taken for M was a 100 cub. centims. flask. The water in A was each time filled up to *a*. Only those two of the values of gt which gave the most widely differing results in the preceding Table were reexamined, viz. $gt=0\cdot33$ and $gt=1\cdot00$. A thread was fastened to the end of the siphon D, so as to deliver its contents in a series of very rapid and minute drops.

TABLE β.

From glass, air-bubbles through water.
T=23° C.
B=767 millims.

gt.	Number of bubbles in 100 cub. centims.	Mean absolute volume of single bubble.
		cub. centim.
$\overset{\prime\prime}{0}$·33	1970, 1970	0·05076
1·00	1974, 1972	0·05068

Hence, under these conditions, rate has little or no influence upon bubble-size. In order to see whether a tube of different calibre would give rise to bubbles more sensitive in regard to their rate, a narrower orifice at *h* was employed. The flask M had a capacity of 50 cub. centims. The following mean results were obtained, each mean being derived from two experiments:—

gt.	Number of bubbles in 50 cub. centims.	Absolute volume of single bubble.
		cub. centim.
$\overset{\prime\prime}{0}$·33	1927	0·02595
1·00	1945	0·02571

This result, taken together with Tables α and β, shows how small is the effect of rate upon bubble-size. If anything, there is, on the whole, a very slight tendency to diminution in bubble-size as *gt* diminishes—that is, as the rate increases. This is just the reverse of what was found to be the case with SLG. Most probably, however, this effect is not due specifically to the rate, but to the alteration in the diameter of the orifice at different rates. When a rapidly succeeding series of bubbles passes through the orifice *h*, the sides of the delivery-tube are swept more completely dry than when the bubbles pass more slowly; so that in the former case the opening is, in fact, a little larger than in the latter. We shall see in the sequel how sensitive bubble-size is to variation in the width of the delivery-tube.

It may be here noticed that, unless the tube H remains strictly in the same position, it is hopeless to attempt to get uniformity in results. This is especially the case when the opening *h* is turned half up in the shape of a siphon; for then the least displacement out of the vertical causes virtually an alteration in the available size of the opening, and a consequent variation in bubble-size. A great and otherwise unaccountable variation in the bubble-number, under circumstances apparently identical, directed attention to this source of error. By taking a wider tube, and allowing the end to contract in the blowpipe flame, a rounded opening is produced, the horizontal projection of which is much less variable with alteration in the verticalness of the tube H.

The reason why bubbles are less sensitive to variations in gt than are drops is sufficiently obvious. In the case of SLG, variation in gt affects the size of a drop by varying the thickness of the liquid-film which covers the solid at the moment of the drop's separation. We have seen that when this film is thin, in consequence of slowness in the supply of liquid to the solid, the size of the drop is diminished, because the solid reclaims liquid from the drop-root at the instant of the latter's departure. But in the case of a bubble, at least in the arrangement of the above experiments, there is in all cases an indefinitely great aëriform residue, the separation of the bubble being determined by the superior density of the liquid medium, and by its persistent cohesion.

Rate being thus of no appreciable influence upon bubble-size, we are not compelled to take the same extreme precaution to ensure uniformity of gt, as was found necessary with drops.

Effect of change in the chemical nature of the bubble-gas.—The gases examined were hydrogen, oxygen, nitrogen, carbonic acid, and atmospheric air. Boiled water was left for several hours in the bags containing the gases, so that the gases might be perfectly saturated with water, and the water with the gases, and so that they might have the same temperature. This water was then employed to fill the vessel A. By this means all disagreement between the volume of the bubbles and the volume of the water flowing from D, caused by the solution of the gas in the water of A, is avoided. In each case the gas was allowed to bubble through G until the water in it was saturated.

The following Table shows the results obtained :—

Column 1. The gas employed.

Column 2. The number of bubbles having together the volume 50 cub. centims., each number being the mean of three experiments.

Column 3. The absolute mean volume of a single bubble.

TABLE γ.

M=50 cub. centims.
$gt=0''\cdot33$.
T=24° C.
B=766 millims.

1. Bubble-gas.	2. Mean number of bubbles having the volume 50 cub. centims.	3. Absolute mean volume of single bubble.
		cub. centim.
Nitrogen	2173·0	0·023009
Air................	2070·0	0·024154
Carbonic acid........	2035·0	0·024570
Oxygen	2021·7	0·024731
Hydrogen	1981·3	0·025235

The chemical nature of the gas, therefore, has also very little influence upon bubble-size. The two purely physical influences active in determining the bubble-size are the density of the gas and its solubility in water. These act to produce opposite effects. Increase of density in the gas delays the departure of the bubble, and thereby increases its size; increase in the solubility of the gas in water impairs the stubborn cohesion of the water, and thereby diminishes the bubble-size. If p and q be the specific gravities of two gases P and Q referred to water, the buoyancy of two equal bubbles of them will be respectively

$$W - pW,$$
$$\text{and } W - qW,$$

where W is the weight of an equal volume of water; that is, $W(q-p)$ is the difference in buoyancy of the two bubbles. The gases arranged in their order of density are

CO_2, O, Air, N, H.

Arranged in order of solubility (at 20° C.),

CO_2, O, H, Air, N.

The properties density and solubility are of course incommensurable, so that we cannot predict the extent to which they may counteract one another in the same gas to determine its bubble-size. But the order of the gases in Table γ is quite consistent with our previous knowledge. Thus the bubble-size of air is intermediate between the bubble-sizes of nitrogen and oxygen. It would, however, at present be premature to attempt to make use of bubble-size to furnish an additional equation in gas-analysis.

Effect of temperature and of tension.—The first of these has also a twofold action, by changing the density of the gas, and by changing the cohesion of the liquid. Within a natural range of 10° C. change of temperature takes no appreciable effect upon bubble-size. Also a variation of three-quarters of an inch in the natural barometric mercurial column is without sensible influence. These two influences were not made matters of special study, but were only examined with the view of ensuring absence of error from other experiments.

Effect of change in the geometrical distribution of solid : size of orifice.—The change examined in this sense was the alteration in the size of the orifice through which the gas bubbled. For this purpose the ends of six tubes of various internal diameter were ground flat, and until they had exactly the same length. One end of each tube was stopped by a little glass disk covered with a film of wax. The tube was then filled to overflowing with distilled water, and another little disk was pressed on the top, the superfluous water being wiped off. The tube was then weighed, emptied, and dried and reweighed. The same being done for each tube, the volumes of the tubes are known to be in the same proportion as the weights of their liquid contents, the diameters or radii of the tubes being in the ratio of the square roots of the same weights. To calibrate tubes in this manner, water is to be preferred to mercury, because the latter leaves a film of air between itself and the glass, and thereby introduces a considerable error in the deduced calibre of very narrow tubes. The tubes were inserted

into the cork of the tube G, fig. A. The vessel M was a burette graduated into tenths of cubic centimetres. A hundred bubbles at $gt=2''\cdot0$ were allowed to pass through G, and the water from D was measured.

Table δ.

$gt=2''\cdot0$.

Relative areas of sections of tubes.	Mean volume of 100 bubbles.	Relative radii of tubes.	Actual observed volumes of 100 bubbles.	
	cub. centims.			
0·0204	3·5	0·1428	3·5	3·5
0·2112	14·9	0·4595	14·9	14·9
0·3642	15·2	0·60348	15·1	15·3
1·9880	17·8	1·4099	17·9	17·7
3·1002	24·4	1·7607	24·3	24·5
4·4094	31·9	2·0998	31·9	31·9

From this Table we see that the bubble-size is very sensitive to the size of the orifice. The bubble-size is doubled if the radius of the orifice is increased fivefold; and so on. The same effect can also be well shown in a manner quite analogous to that adopted * to show the effect of variation in radius of curvature of the solid (SLG).

If the same quantity of gas be made to bubble in succession through the same liquid, similarly disposed in similar vessels, and if the tubes through which it is delivered have continually decreasing diameters, then the rates of bubbling are seen to follow the inverse order of the diameters of the tubes. Fig. D shows such an arrangement, which requires no explanation. In fact the reason why increase in radius of curvature in the case SLG produces increase of drop-size is very similar to that which causes increase of orifice to increase bubble-size in the case SGL. In the former case the thickness and general approximation of the residual liquid-film to the drop is greatest in large and flat surfaces; in the latter the area of residual gas is larger when the orifice is larger. When, around a large orifice, the liquid medium closes upon the bubble, the latter is not so straitened for material as when the orifice is narrow.

The influence of the size of the tube upon bubble-size is of considerable practical importance. In washing a gas, in separating two gases from one another by a medium which absorbs one of them, in saturating a liquid by a gas (a process which so often occurs in manufactures and analysis), the completeness of the operation invariably depends upon the extent of surface in common between the gas and liquid during a given time. If a spherical bubble, having the volume V and the surface S, be divided into two equal spherical bubbles, each having the volume $\frac{V}{2}$ and the surface s, then

$$\frac{S}{2s}=\frac{1}{2^{\frac{1}{3}}}.$$

* On Drops, p. 460.

So that if the surface of the original bubble be 1, the surface of the two bubbles of half the size taken together is 1·259885. By making the gas-delivery-tube small, the absorbent surface of the same quantity of gas which passes through is increased in this manner, and the absorption is consequently more rapid or more complete.

Effect of change in the chemical nature of the liquid medium.—To examine this (perhaps the most interesting phase of the causes of variation in bubble-size), the gas-bag L was replaced by a chloride-of-calcium tube. The cotton-wool of J was saturated with the liquid, which was placed for examination in G ; so that the bubbling gas was dry air already saturated with the vapour of the liquid through which it had to bubble. It is clear that if the air so charged were to come into contact with the water in A, the vapour would dissolve in the water, while the air would become moist; a difference in volume would be thereby occasioned, according to the difference of tension of the vapour of the liquid in G and J and that of water. To avoid this source of error, the vessel A was filled with mercury. After each experiment the vessel A was completely refilled with mercury, so as to expel the vapour of the liquid employed in the previous experiment. The mercury was then run off at D, until it fell in A nearly to the mark a. The liquid under examination in G had a height above h inversely as its specific gravity : this the graduation of the tube G made easy. By this means the pressure on the gas as it issued from h was the same in all the experiments. The vessels A, G, and J were all sunk in the same trough of water, so that the volume of the air should undergo no alteration from temperature, either during or after its passage through G. When gt had been brought exactly to 2″, and the mercury in A had sunk to a, a graduated burette was brought under the end of the siphon D, and kept there while 100 bubbles passed through G. The numbers of column 2 are each of them the mean of two determinations.

TABLE e.

$gt=2''$.
T=25° C.
B=764 millims.

Liquid medium.	Mean absolute volume of 100 bubbles of air.
	cub. centims.
Mercury........	41·2
Glycerine	11·45
Water..........	8·60
Butyric acid	5·82
Acetic acid......	5·72
Alcohol	4·80
Benzol	4·80
Turpentol	4·53
Acetic ether	3·72

These liquids, which were purposely taken the same as those whose drop-sizes were examined, are arranged in Table ϵ in the order of the magnitude of the bubble-size. We see that the order is not the same in the two cases. The difference is due to the elimination in Table ϵ of the influence of gravitation. In fact the only forces which influence bubble-size, as shown in Table ϵ, are the retentive and stubborn cohesions of the liquid*; for the first of these seeks to diminish, the second to increase the bubble-size. If RC be the retentive, and SC the stubborn cohesion, the liquids are arranged in Table ϵ in the same order of magnitude as are the values of $\frac{SC}{RC}$. The density of a liquid seems therefore to vary with its stubborn rather than with its retentive cohesion; for there is an evident general tendency in the above Table ϵ for the liquids to arrange themselves in the order of their specific gravities. Water once more distinguishes itself, taking a higher place in the scale than its density would point to: this must arise either from its exceptionally great stubborn, or from its exceptionally small retentive cohesion.

Acetic ether and alcohol are also exceptional—the former taking a lower, the latter a higher place in the scale than would be the case if the same state of quantity of matter in a given space (which is usually measured by means of gravity) affected also the cohesion of the liquid so as alone to determine the bubble-size of a gas passing through it. Perhaps also the gas having different degrees of solubility in the different liquids may affect their cohesions unequally. This source of variation, however, is probably very small, as we have seen to be the case when the gas varies and the liquid remains the same. A few experiments with a mixture of benzol and turpentol, and with alcohol and water, showed that in all cases the mixed liquid gives rise to a bubble intermediate in size between those caused by the single liquids.

By measuring the volume of a greater number of bubbles, the actual differences of bubble-size due to various liquids would of course become more apparent.

Throughout the examination of drops and bubbles in the present and previous communications, I have sought to direct attention to the main influences which fix the size of a drop or bubble, rather than to pursue any one branch of the inquiry into its minute ramifications. Further, the subject has been treated wholly from a statical point of view; that is, the bubble and drop have been considered at that period of their being when the contending forces which act upon them have brought them into a state of unstable equilibrium or incipient motion. It is in fact only at this point, the instant of their ripeness, that they have a definite size; for their size increases until the contending forces themselves withdraw the drop or bubble from the sphere of the action which determines their volume.

Knowing now the direction and approximately the relative amounts of the effects due to the various conditions under which the drop and bubble

* For the meaning of these terms see Paper "On Drops," p. 469.

are formed, the most prolific field of inquiry is promised by the study of the drop- and bubble-size as a means of proximate chemical diagnosis*. It does not appear that bubble-volume is at present likely to afford an additional equation for gas-analysis; but we have seen that both drop-size and bubble-size may offer very valuable criteria as to the constitution of liquids. And although the former (drop-size), especially in the case SL_1L_2, is by far the most sensitive to variation in the chemical constitution and proportion of mixed constituents, the latter has the advantage of requiring a much less amount of liquid, and of being applicable to every liquid without regard to its solubility in other liquids.

II. "Note on the Invisible Radiation of the Electric Light." By JOHN TYNDALL, F.R.S. Received January 13, 1865.

Pending the preparation of my complete memoir, which may occupy me for some time to come, I would ask permission of the Royal Society to lay before the Fellows a brief and partial summary of the results of my experiments on the invisible radiation of the electric light.

The distribution of heat in the spectrum of the electric light was examined by means of the linear thermo-electric pile, applied to the solar spectrum by Melloni, Franz, Müller, and others. The electric spectrum was formed by lenses and prisms of pure rock-salt, its width being equal to the length of the row of elements forming the pile. The latter, standing at right angles to the length of the spectrum, was caused to pass through its various colours in succession, and to search the spaces beyond the region of colour, in both directions.

As in the case of the solar spectrum, the heat was found to augment from the violet to the red, while the maximum heating effect was observed beyond the red, and at a distance from the red, in one direction, equal to that of the green of the spectrum in the other.

The augmentation of temperature beyond the red in the case of the electric light is sudden and enormous. Plotting from a datum line the thermal intensity of the various portions of the spectrum, the ordinates suddenly increase in length beyond the red, reach a maximum, and then fall somewhat more suddenly on the other side. When the ends of the ordinates are united, the curve beyond the red rises in a steep and massive peak, which quite dwarfs the luminous portion of the spectrum.

The comparative height and steepness of this peak are much greater than those obtained by Professor Müller for the solar spectrum. Aqueous vapour acts powerfully upon the invisible rays; and doubtless the action of this substance in our atmosphere has toned down the eminence beyond the red in Professor Müller's diagram. A solar spectrum, produced beyond

* Some word is required to denote the acquirement of the knowledge of the constitution of a substance without taking it to pieces (analysis). "Diagnosis," used in its purely etymological sense, answers this purpose.

the limits of the atmosphere, would probably exhibit as steep a peak as that of the electric light.

In the experiments now to be referred to, the rays from the electric light were converged by a small concave mirror. The glass mirror silvered at the back, which usually accompanies the camera of Duboscq's electric lamp, was one of the first employed. It was brought so near the electric light as to cast an image of the coal-points five or six inches in advance of the light. A solution of iodine in bisulphide of carbon, contained in a rock-salt cell, was then placed in front of the lamp: the light was thereby cut off; but the focus of dark rays remained, and various effects of combustion and incandescence were obtained at the focus. A mirror 4 inches in diameter, and silvered in front, will enable an experimenter to obtain most, if not all the results now to be mentioned. I also employ a mirror 8 inches in diameter, and having a focal length of 8 inches, with excellent effect.

It is not necessary to enclose the opaque solution in a rock-salt cell. The vessel intended for a solution of alum, which usually accompanies the lamp of Duboscq, and the sides of which are of glass, answers admirably. It is, however, not quite deep enough for the several tests to which I have subjected it, and in crucial experiments I employ a deeper vessel with rock-salt sides.

With the 8-inch mirror just referred to behind the electric light, the opaque solution in front, and the focus of invisible rays about 6 inches distant from the electric light, the following effects have been obtained:—

1. Wood, painted black, when brought into the dark focus, emits copious volumes of smoke, and is soon kindled at the two spots on which the images of the two coal-points fall.

2. A piece of brown paper placed near the focus soon shows a burning surface, which spreads over a considerable space, the paper finally bursting into flame.

3. Black paper brought into the focus is immediately inflamed.

4. The wood of a hat-box similarly placed is rapidly burnt through, and usually bursts into flame.

5. The end of a cigar, placed at the dark focus, is instantly ignited.

6. Disks of charred paper placed in the focus are raised to brilliant incandescence, surfaces of considerable extent being brought to a vivid glow. Charcoal is also ignited.

7. A piece of charcoal, suspended in a receiver of oxygen, is ignited in the dark focus and caused to burn brilliantly, the rays after crossing the glass of the receiver being still sufficiently powerful to heat the coal up to incandescence.

8. A mixture of oxygen and hydrogen is exploded in the dark focus by the ignition of its envelope.

9. A piece of zinc foil, blackened on one side to diminish reflexion, is pierced and inflamed. By gradually drawing the strip, once inflamed, across the focus, it may be kept blazing for a considerable length of time. This is a particularly beautiful experiment.

10. Magnesium wire, presented suitably to the focus, burns with its intensely luminous flame.

In all these cases the effect was due, in part, to chemical action; this, however, may be excluded.

11. A plate of any refractory metal, sufficiently thin, and with its reflective power suitably diminished, is raised to incandescence in the dark focus. Gold, silver, copper, aluminium, and platinum have been thus rendered incandescent.

12. Platinized platinum shows the effect best: in a thin leaf it may be rendered white-hot, and on it is depicted an incandescent image of the coal-points. When the points are drawn apart, or caused to approach each other, their incandescent images conform to their motion.

The assemblage of phenomena here described, and others to be referred to in my completed memoirs, may, I think, be properly expressed by the term *Calorescence*. This word involves no hypothesis, and it harmonizes well with the term fluorescence, now universally employed with reference to the more refrangible end of the spectrum*.

III. "Note on a New Object-glass for the Microscope, of higher magnifying power than any one hitherto made." By Lionel S. Beale, M.B., F.R.S., F.R.C.P., Professor of Physiology and of General and Morbid Anatomy in King's College, and Physician to King's College Hospital. Received December 30, 1865.

I desire to record the completion of a new objective, with a magnifying power double that of the twenty-fifth. This glass is a fiftieth, and magnifies nearly three thousand diameters with the low eyepiece. Messrs. Powell and Lealand, the makers, to whom science is indebted for this the highest power yet made, produced a sixteenth in the year 1840, and the twenty-sixth in 1860.

The fiftieth defines even better than the twenty-fifth, which is now made instead of the twenty-sixth. Plenty of light for illuminating the objects to be examined is obtained by the use of a condenser provided with a thin cap, having an opening not more than the $\frac{1}{30}$th of an inch in diameter. The preparation may be covered with the thinnest glass made

* On the 5th of last December I tried the passage of the rays from the electric lamp through a great number of differently coloured glasses. Incandescence was obtained through almost all of them; and in one instance, the radiation passing through a blue glass, the thermograph of the coal-points was *of a pink colour*. A thick black glass, obtained from Mr. Ladd, when held in front of the lamp, was found to be not perfectly opaque; still the platinum could not be raised to incandescence a tall when placed in the focus. Being called away from the Royal Institution early in the afternoon, I gave directions to my assistant, Mr. Barrett, to continue the experiments. He informs me that on placing in the path of the rays a combination of two thin plates of black glass, one transmitting a whitish-green, and the other a deep red, the light was entirely intercepted, and feeble though distinct incandescence was obtained at the focus. With radiation through the solution of iodine, the thermograph on this day rose to a white heat.

by Messrs. Chance, of Birmingham, or mica, and there is plenty of room for focusing to the lower surface of thin specimens, which can alone be examined by high powers as transparent objects. I beg to draw attention to these very high powers at this time more particularly, because the facts recently urged in favour of the doctrine of spontaneous generation lately revived may be studied with great advantage. Not only are particles, too small to be discerned by a sixteenth, well seen by a twenty-fifth or a fiftieth, but particles too transparent to be observed by the twenty-fifth are distinctly demonstrated by the fiftieth. I feel sure that the further careful study, by the aid of these high powers, of the development and increase of some of the lowest organisms, and the movements which have been seen to occur in connexion with certain forms of living matter (Amœba, white blood-corpuscle, young epithelial cells, &c.), will lead to most valuable results bearing upon the much debated question of *vital actions.*

Another very great advantage resulting from the use of the highest powers occurs in minute investigations upon delicate structures which occupy different planes, as is the case in many nervous organs. In studying the distribution of the nerves in some of the peripheral organs of vertebrate animals, very fine fibres can be followed as they lie upon different planes.

The most delicate constituent nerve-fibres of the plexus in the summit of the papillæ of the frog's tongue (New Observations upon the Minute Anatomy of the Papillæ of the Frog's Tongue, Phil. Trans. for 1864), can be readily traced by the aid of this power. The finest nerve-fibres thus rendered visible are so thin, that in a drawing they would be represented by fine single lines. Near the summit of the papilla there is a very intricate interlacement of nerve-fibres, which, although scarcely brought out by the twenty-fifth, is very clearly demonstrated by this power. In this object the definition of the fibres, as they ramify in various planes one behind another, is remarkable; and the flat appearance of the specimen as seen by the twenty-fifth, gives place to that of considerable depth of tissue and perspective. The finest nerve-fibres ramifying in the cornea and in certain forms of connective tissue are beautifully brought out by this power, and their relation to the delicate processes from the connective-tissue corpuscles can be more satisfactorily demonstrated than with the twenty-fifth. The advantage of the fiftieth in such investigations seems mainly due to its remarkable power of penetration. The angular aperture of this glass is 150°. Many twelfths have been made with a higher angular aperture, amounting to 170°.

It should be stated that the specimens of animal tissues which I have subjected to examination by very high powers are mounted in strong syrup, or in the strongest glycerine, according to the process detailed in 'How to work with the Microscope,' 3rd edition, p. 204. It is perfectly true that no advantage results from examining by the aid of very high powers the tissues of man and the higher animals immersed in water, or in fluids of which water is the chief constituent; nor is it possible to make the specimen sufficiently

thin for examination with very high powers if immersed in a limpid fluid. The arrangement of the nerve-fibres I have referred to is not to be demonstrated in tissues immersed in water. The finer branches of the nerves are in fact quite invisible until the specimen is well impregnated with a highly refracting fluid. The imperfect methods of preparation usually employed for examining the higher tissues have given rise to a prejudice against the employment of high powers. I have not referred to the use of very high powers in studying the characters of the Diatomaceæ, because it is a branch of microscopic investigation which I have very little studied. Other observers will probably state very shortly if anything is gained by the use of these high powers in this department. It is probable that, by improved means in illuminating the objects, many new and important points will be made out by the use of very high magnifying powers. Mr. Brooke has already suggested possible improvements in the condenser, some of which, I believe, he will carry out shortly.

January 26, 1865.

Major-General SABINE, President, in the Chair.

The following communications were read :—

I. "Researches on Solar Physics.—Series I. On the Nature of Solar Spots." By WARREN DE LA RUE, Ph.D., F.R.S., BALFOUR STEWART, A.M., F.R.S., Superintendent of the Kew Observatory, and BENJAMIN LOEWY, Esq. Received January 10, 1865.

(Abstract.)

After giving a short sketch of the history of their subject, the authors proceed to state the nature of the materials which had been placed at their disposal. In the first place, Mr. Carrington had very kindly put into their hands all his original drawings of sun-spots, extending from November 1853 to March 1861. In the next place, their materials were derived from the pictures taken by the Kew heliograph. A few pictures were taken by this instrument at Kew Observatory in the years 1858 and 1859. In July 1860 it was in Spain doing service at the total eclipse. In 1861 a few pictures were taken at Kew, while from February 1862 to February 1863 the instrument was in continuous operation at Mr. De la Rue's private observatory at Cranford, and from May 1863 until the present date it has been in continuous operation at Kew under Mr. De la Rue's superintendence. A Table was then given, from which it was deduced that the number of groups observed at Kew from June to December 1863 inclusive was 64, while that observed by Hofrath Schwabe during the same interval was 69. In like manner, the number at Kew between January and November 1864 inclusive was 109, while during the same interval Hofrath Schwabe observed 126. It thus appears that Schwabe's numbers are somewhat larger than those of Kew; but probably, by means of a constant corrective, the one series may be made to dovetail with the other.

The authors then attempted to answer the following questions:—

(1) Is the umbra of a spot nearer the sun's centre than the penumbra, or, in other words, is it at a lower level?

(2) Is the photosphere of our luminary to be viewed as composed of heavy solid, or liquid matter, or is it of the nature either of a gas or cloud?

(3) Is a spot (including both umbra and penumbra) a phenomenon which takes place beneath the level of the sun's photosphere or above it?

In answering the first of these, it was shown that if the umbra is appreciably at a lower level than the penumbra, we are entitled to look for an apparent encroachment of the umbra upon the penumbra on that side which is nearest the visual centre of the disk. This, in fact, was the phenomenon which Wilson observed, and which led him to the belief that the umbra was nearer the sun's centre than the penumbra.

Two Tables are then given, showing the relative disposition of the umbra and penumbra for each spot of the Kew pictures available for this purpose.

In the first of these, this disposition was estimated from left to right, this being the direction in which spots advance across the visible disk by rotation; while in the second Table this disposition was estimated in a direction parallel to circles of solar longitude, and in this Table only spots having a high solar latitude were considered.

From the first of these Tables it was shown that, taking all those cases where an encroaching behaviour of the umbra in a right and left direction has been perceptible, 86 per cent. are in favour of the hypothesis that the umbra is nearer the centre than the penumbra, while 14 per cent. are against it. It also appeared that, taking *all available spots* and distributing them into zones according to their distance from the centre, this encroaching behaviour is greatest when spots are near the border, and least when they are near the centre.

From the second Table, in which only spots of high latitude were considered, it was shown that, taking all those cases where an encroaching behaviour of the umbra in an up-and-down direction has been perceptible, 80·9 per cent. are in favour of the hypothesis that the umbra is nearer the centre than the penumbra, while 19·1 per cent. are against it.

The result of these Tables is therefore favourable to this hypothesis.

The authors next endeavoured to answer the following question:—Is the photosphere of our luminary to be viewed as composed of heavy solid, or liquid matter, or is it of the nature either of a gas or cloud?

It was observed that the great relative brightness of faculæ near the limb leads to the belief that these masses exist at a high elevation in the solar atmosphere, thereby escaping a great part of the absorptive influence, which is particularly strong near the border; and this conclusion was confirmed by certain stereoscopic pictures produced by Mr. De la Rue, in which the faculæ appear greatly elevated. It was remarked that faculæ often retain the same appearance for several days, as if their matter were capable of remaining suspended for some time.

A Table was then given, showing the relative posit on of sun-spots and

their accompanying faculæ for all the Kew pictures available for this purpose.

From this it appeared that out of 1137 cases 584 have their faculæ entirely or mostly on the left side, 508 have it nearly equal on both sides, while only 45 have it mostly to the right. It would thus appear as if the luminous matter being thrown up into a region of greater absolute velocity of rotation fell behind to the left; and we have thus reason to suppose that the faculous matter which accompanies a spot is abstracted from that very portion of the sun's surface which contains the spot, and which has in this manner been robbed of its luminosity.

Again, there are a good many cases in which a spot breaks up in the following manner. A bridge of luminous matter of the same apparent luminosity as the surrounding photosphere appears to cross over the umbra of a spot unaccompanied by any penumbra. There is good reason to think that this bridge is above the spot; for were the umbra an opaque cloud and the penumbra a semi-opaque cloud, both being above the sun's photosphere, it is unlikely that the spot would break up in such a manner that the observer should not perceive some penumbra accompanying the luminous bridge. Finally, detached portions of luminous matter sometimes appear to move across a spot without producing any permanent alteration.

From all this it was inferred that the luminous photosphere is not to be viewed as composed of heavy solid, or liquid matter, but is rather of the nature either of a gas or cloud, and also that a spot is a phenomenon existing below the level of the sun's photosphere.

The paper concluded with theoretical considerations more or less probable. Since the central or bottom part of a spot is much less luminous than the sun's photosphere, it may perhaps be concluded that the spot is of a lower temperature than the photosphere; and if it be supposed that all the sun's mass at this level is of a lower temperature than the photosphere, then we must conclude that the heat of our luminary is derived from without.

II. "On the Spectrum of the Great Nebula in the Sword-handle of Orion." By William Huggins, F.R.A.S. Communicated by the Treasurer. Received January 11, 1865.

In a paper recently presented to the Royal Society*, I gave the results of the application of prismatic analysis to some of the objects in the heavens known as nebulæ. Eight of the nebulæ examined gave a spectrum indicating gaseity, and, of these, six belong to the class of small and comparatively bright objects which it is convenient to distinguish still by the name of planetary. These nebulæ present little indication of probable resolvability into discrete points, even with the greatest optical power which has yet been brought to bear upon them.

The other two nebulæ which gave a spectrum indicative of matter in the

* On the Spectra of some of the Nebulæ, Phil. Trans. 1864, p. 437.

gaseous form, are 57 M, the annular nebula in Lyra, and 27 M, the Dumb-bell nebula. The results of the examination of these nebulæ with telescopes of great power must probably be regarded as in favour of their consisting of clustering stars. It was therefore of importance to determine, by the observation of other objects, whether any nebulæ which have been *certainly resolved* into stars give a spectrum which shows the source of light to be glowing gas. With this purpose in view I submitted the light of the following easily resolved clusters to spectrum analysis.

"4670. 2120 h. 15 M. Very bright cluster; well resolved" *.

"4678, 2125 h. 2 M. Bright cluster, well resolved."

Both these clusters gave a continuous spectrum.

I then examined the Great Nebula in the Sword-handle of Orion. The results of telescopic observation on this nebula † seem to show that it is suitable for observation as a crucial test of the correctness of the usually received opinion that the resolution of a nebula into bright stellar points is a certain and trustworthy indication that the nebula consists of discrete stars after the order of those which are bright to us. Would the brighter portions of the nebula adjacent to the trapezium, which have been resolved into stars, present the same spectrum as the fainter and outlying portions? In the brighter parts, would the existence of closely aggregated stars be revealed to us by a continuous spectrum, in addition to that of the true gaseous matter?

The telescope and spectrum apparatus employed were those of which a description was given in my paper already referred to.

The light from the brightest parts of the nebula near the trapezium was resolved by the prisms into three bright lines, in all respects similar to those of the gaseous nebulæ, and which are described in my former paper.

These three lines, indicative of gaseity, appeared (when the slit of the apparatus was made narrow) very sharply defined and free from nebulosity; the intervals between the lines were quite dark.

When either of the four bright stars, α, β, γ, δ Trapezii was brought upon the slit, a continuous spectrum of considerable brightness, and nearly linear (the cylindrical lens of the apparatus having been removed), was seen, together with the bright lines of the nebula, which were of considerable length, corresponding to the length of the opening of the slit. The

* The numbers and descriptions are from Sir John Herschel's Catalogue, Phil. Trans. 1864, part 1.

† "The general aspect of the less luminous and cirrous portion is simply nebulous and irresolvable; but the brighter portion immediately adjacent to the trapezium forming the square front of the head, is shown with the 18-inch reflector broken up into masses, whose mottled and curdling light evidently indicates, by a sort of granular texture, its consisting of stars, and when examined under the great light of Lord Rosse's reflector, or the exquisite defining power of the great achromatic at Cambridge, U. S., is evidently perceived to consist of clustering stars. There can therefore be little doubt as to the whole consisting of stars too minute to be discerned individually even with these powerful aids, but which become visible as points of light when closely adjacent in the more crowded parts . . . ."—Sir John Herschel, 'Outlines of Astronomy,' 7th edition, pp. 651, 652.

fifth star γ' and the sixth α' are seen in the telescope, but the spectra of these are too faint for observation.

The positions in the spectra of α, β, γ, δ Trapezii, which correspond to the positions in the spectrum of the three bright lines of the nebula, were carefully examined, but in no one of them were dark lines of absorption detected.

The part of the continuous spectra of the stars α, β, γ, near the position in the spectrum of the brightest of the bright lines of the nebula, appeared on a simultaneous comparison to be more brilliant than the line of the nebula, but in the case of γ the difference in brightness was not great. The corresponding part of δ was perhaps fainter. In consequence of this small difference of brilliancy, the bright lines of the adjacent nebula appeared to cross the continuous spectra of γ and δ Trapezii.

Other portions of the nebula were then brought successively upon the slit; but throughout the whole of those portions of the nebula which are sufficiently bright for this method of observation the spectrum remained unchanged, and consisted of the three bright lines only. The whole of this Great Nebula, as far as it lies within the power of my instrument, emits light which is identical in its characters; the light from one part differs from the light of another in intensity alone.

The clustering stars of which, according to Lord Rosse and Professor Bond, the brighter portions of this nebula consist, cannot be supposed to be invisible in the spectrum apparatus because of their faintness, an opinion which is probably correct of the minute and widely separated stars seen in the Dumb-bell nebula, and to which reference was made in my former paper. The evidence afforded by the largest telescopes appears to be that the brighter parts of the nebula in Orion consist of a "mass of stars;" the whole, or the greater part of the light from this part of the nebula, must therefore be regarded as the united radiation of these numerous stellar points. Now it is this light which, when analyzed by the prism, reveals to us its gaseous source, and the bright lines indicative of gaseity are free from any trace of a continuous spectrum, such as that exhibited by all the brighter stars which we have examined.

The conclusion is obvious, that the detection in a nebula of minute closely associated points of light, which has hitherto been considered as a certain indication of a stellar constitution, can no longer be accepted as a trustworthy proof that the object consists of true stars. These luminous points, in some nebulæ at least, must be regarded as themselves gaseous bodies, denser portions, probably, of the great nebulous mass, since they exhibit a constitution which is identical with the fainter and outlying parts which have not been resolved. These nebulæ are shown by the prism to be enormous gaseous systems; and the conjecture appears probable that their apparent permanence of general form is maintained by the continual motions of these denser portions which the telescope reveals as lucid points.

The opinions which have been entertained of the enormous distances of the

nebulæ, since these have been founded upon the supposed extent of remoteness at which stars of considerable brightness would cease to be separately visible in our telescope, must now be given up in reference at least to those of the nebulæ the matter of which has been established to be gaseous.

It is much to be desired that *proper motion* should be sought for in those of the nebulæ which are suitable for this purpose; indications of parallax might possibly be detected in some, if any nebulæ could be found that would admit of this observation.

If this view of the greater nearness to us of the gaseous nebulæ be accepted, the magnitudes of the separate luminous masses which the telescope reveals as minute points, and the actual intervals existing between them, would be far less enormous than we should have to suppose them to be on the ordinary hypothesis.

It is worthy of consideration that all the nebulæ which present a gaseous spectrum exhibit the *same three bright lines*; in one case only, 18 H.IV., was a fourth line seen. If we suppose the gaseous substance of these objects to represent the "nebulous fluid" out of which, according to the hypothesis of Sir Wm. Herschel, stars are to be elaborated by subsidence and condensation, we should expect a gaseous spectrum in which the groups of bright lines were as numerous as the dark lines due to absorption which are found in the spectra of the stars. Moreover, if the improbable supposition be entertained, that the three bright lines indicate matter in its most elementary forms, still we should expect to find in some of the nebulæ, or in some parts of them, a more advanced state towards the formation of a number of separate bodies, such as exist in our sun and in the stars; and such an advance in the process of formation into stars would have been indicated by a more complex spectrum.

My observations, as far as they extend at present, seem to be in favour of the opinion that the nebulæ which give a gaseous spectrum, are systems possessing a structure, and a purpose in relation to the universe, altogether distinct and of another order from the great group of cosmical bodies to which our sun and the fixed stars belong.

The nebulous star ι Orionis was examined, but no peculiarity could be detected in its continuous spectrum*.

III. "Further Observations on the Planet Mars." By John Phillips, M.A., LL.D., F.R.S., F.G.S., Professor of Geology in the University of Oxford. Received January 12, 1865.

The return of Mars to his periodical opposition with the sun has enabled me to offer a few observations on this planet, in addition to those which on a former occasion I had the honour to present to the Society †. Among

* Admiral Smyth appears to have always maintained that the results of telescopic observation on the nebulæ were insufficient to support the opinion that all these objects were probably of stellar constitution. See his 'Cycle of Celestial Objects,' vol. i. p. 316; and his 'Speculum Hartwellianum,' pp. 111–114.

† Proceedings of the Royal Society, 1863.

the subjects then suggested for consideration was the permanence of the main features of light and shade which had been recognized by many observers. Another question requiring attention referred to the fogginess or seeming cloudiness of the planet, also noticed by many observers, some of whom represented what might be thought effects of currents in the atmosphere round him. Again, it was a matter for further research whether the colours of what we suppose to be land and sea (the reddish hue of the land, and the grey aspect of the sea) were capable of explanation by any peculiarity of the soil or atmosphere, and whether, from the phenomena of snows visible about the poles and elsewhere, the climate of Mars could be estimated on trustworthy grounds.

My observations are too few to furnish answers for all these questions; but I have something to say in reply to some of them, though the distance of Mars from the Earth during the late opposition was too great to allow of such close scrutiny as in 1862.

First, then, in respect of the permanence of the main features of the planet. I submit several drawings* made between the 14th November and 13th December (both inclusive), the dates being marked on each, for comparison with others made in 1862, partly by Mr. Lockyer, partly by myself; from which it will immediately appear that no appreciable change has occurred in the main outlines of land and sea, in the longitudes observed. A certain fogginess has been noticed, especially on the 18th and 20th November, such as does not commonly occur with Jupiter or Saturn; but it seemed to be due to no essential circumstance of the planet, for it grew less and less as the observation approached the meridian.

The colour of the larger masses of land is the same as formerly observed, but fainter from distance; and the sea is grey and shadowy, but without the very distinct greenish hue which was noticed in 1862. Finally, the snows round the south pole appeared much less extensive than in 1862, and were not really observable with distinctness except on a few evenings. Snowy surfaces, scarcely more defined, but much more extensive, were observed in parts of the northern regions, not immediately encircling the pole (which was invisible), but in two principal and separate tracts estimated to reach 40° or 45° from the pole. On one occasion (30th November) two practised observers (Mr. Luff and Mr. Bloxidge) noticed with me one of these gleaming masses of snow, very distinct—so much so, that, as happened with the south polar snow in 1862, it seemed to project beyond the circular outline—an optical effect, no doubt, and due to the bright irradiation. This white mass reached to about 40° or 45° from the pole, in the meridian of 30° on my globe of Mars. Another mass was noticed on the 14th and 18th November, in long. 225°, and extending to lat. 50°. In each case the masses reached the visible limb.

The small extent of the snow visible at the further pole may be truly the effect of the position of the planet. If we remember that on this occasion the axis of Mars was nearly (within about 6° or 8°) at right angles

* Preserved for reference in the Archives; an equatorial projection is given in Plate II.

to the line of sight, while in 1862 it was oblique (about 26°), we shall perceive that though the snow about the south pole were really as extensive in 1864 as it appeared to be in 1862, it could not possibly appear even nearly so large, and in fact could barely be seen (as it was) under the very small angle which it would subtend on the limb. There may, however, have been really less snow round the south pole, in consequence of the longer action of the summer heat on Mars in 1864 than in 1862.

The ruddy tint of the surface of the broad tracts of land is so constantly observed in these parts as to claim to be regarded as characteristic of some peculiarity in them—some special kind of terrestrial substance for example *. On the other hand, the tint is so much like that of our evening clouds as to suggest the probability of its being due to the deep atmospheric zone which has been often ascribed to this planet, though perhaps, until of late years, on insufficient grounds †. On this head spectral analysis will probably enlighten us. If, however, there be such a deep covering of atmosphere, it might explain some facts regarding the climate which otherwise appear unaccountable. Some considerable amount of vaporous atmosphere there must be, to give origin to the beds of snow which alternately invest and desert the opposite poles, if indeed either pole be ever quite free from snow.

In different Martial years the extent of the snow appears nearly the same under nearly similar conditions. Compare, for instance, Herschel's drawing for August 16, 1830 ‡, with my sketch for September 27, 1862 §, and that now presented for November 20, 1864.

Snows appear to have been observed in mass as far from the south pole as lat. 40°. This occurred in April 1856, according to a drawing by Mr. De la Rue: snow in lat. 50° or perhaps 45° North is the result of my observations during this late opposition. Assuming this to be the geographical limit of the freezing mean winter temperature, we see at once that it differs but little from that of the earth, on which the isothermal line of 32° varies, according to local peculiarities, from the latitude of 40° to that of 60°. If the snows on the land of Mars be compared with those on the northern tracts of Asia and America, they will be found not to extend further. And as the snows, if they do not actually disappear, are reduced to small areas about either pole in *its* warm season, thus showing the mean summer temperature there to be not less than 32°, this confirms the general impression that the variations of the climate of Mars are comprised

* "In this planet we discern, with perfect distinctness, the outlines of what may be continents and seas. Of these, the former are distinguished by that ruddy colour which characterizes the light of this planet (which always appears red and fiery), and indicates, no doubt, an ochrey tinge in the general soil, like what the red sandstone districts on the earth may possibly offer to the inhabitants of Mars, only more decided. Contrasted with this (by a general law in optics) the seas, as we may call them, appear greenish."—Herschel's Astronomy (ed. 1833), p. 279.

† "It has been surmised to have a very extensive atmosphere, but on no sufficient or even plausible grounds."—*Ibid.* p. 279, note.

‡ Treatise on Astronomy (ed. 1833), pl. 1.

§ Proc. Roy. Soc. 1863.

within nearly the same thermic limits as those of the earth. In all the broad belt of 30° or 40° from the equator, the temperature seems to be such as always to allow of evaporation; between that limit and the pole, snows gather and disperse according to the season of the year, while for about 8° or 10° more or less round the pole, the icy circle seems to be perennial.

The relative mean distances from the Sun of Mars and the earth being taken at 100 and 152, the relative solar influence must be on Mars 100 to 231 on the earth; so that the surface of the more distant planet might rather be expected to have shown signs of being fixed in perpetual frost, than to have a genial temperature of 40° to 50°, if not 50° to 60°, as the earth has, taken on the whole. How is this to be accounted for? Of two conceivable influences which may be appealed to, viz. very high interior heat of the planet, and some peculiarity of atmosphere, we may, while allowing some value to each, without hesitation adopt the latter as the more immediate and effective.

To trace the effects in detail must be impracticable; but in the general we may remark that as a diminution of the mass of vaporous atmosphere round the earth would greatly exaggerate the difference of daily and nightly, and of winter and summer temperature, so the contrary effect would follow from an augmentation of it. Applying this to Mars, we shall see that his extensive atmosphere would reduce the range of summer and winter, and of daily and nightly temperature. It would, moreover, augment the mean temperature by the peculiar action of such an atmosphere, which, while readily giving passage to the solar rays, would resist the return of dark heat-rays from the terrestrial surface, and prevent their wasteful emission into space*. This effect obtains now on the earth, which is rendered warmer, as well as more equable in temperature, by the atmosphere than it would be without it. It is conceivable that it may obtain upon Mars to a greater degree, even without supposing the atmosphere to be materially different in its nature from that round the earth, or the surface of Mars to have any specially favourable or exceptional characters for the absorption and radiation of heat. It seems, however, requisite to suppose a greater communication of heat from the interior of the planet; for otherwise the additional vapour, to which the warming effect is in the main to be ascribed, could not probably be supported in the atmosphere. On the whole we may, perhaps, be allowed to believe that Mars is habitable.

Here, so far as direct observations upon the aspect of Mars are available, we may pause. The researches of the Radcliffe Observer, lately in Oxford, and formerly at Greenwich, have, however, brought into view a peculiarity in the constitution of this planet which deserves special notice. Its figure is spheroidal, as might be expected from the general laws of planetary form; but it is spheroidal in so high a degree as to be quite exceptional in this respect. Computing by the known rotation-velocity, and the admitted measures and mass of Mars, its ellipticity should be about $\frac{1}{300}$.

* Life on the Earth, 1860, p. 163–65. Tyndall's Researches, Proceedings of the Royal Society, February 1861.

Mr. Main's observations with the splendid Oxford Heliometer give as the most probable result, the large fraction of $\frac{1}{37 \cdot 59}$ for 1862. This excellent astronomer has continued his observations during the late opposition. My own attempts to obtain the ellipticity with the micrometer eyepiece reading to $0'' \cdot 2$ of arc failed to give satisfactory measures. The ellipticity, indeed, seemed to be small, and was merely observable, not really measurable or even to be approximately estimated by the help of this apparatus.

IV. "Notices of the Physical Aspect of the Sun." By John Phillips, M.A., LL.D., F.R.S., F.G.S., Professor of Geology in the University of Oxford. Received January 13, 1865.

Part I.

Frequently, during many years, the peculiarities of the physical aspect of the sun have arrested my attention, and induced me often to sketch and sometimes to measure; but until Mr. Cooke furnished me with the accurate and convenient equatorial which I now employ, there seemed little hope of my being able to draw correctly or observe systematically. During some late occasions I have endeavoured to obtain trustworthy representations not only of some of the darker tracts, denominated "spots," and the brighter parts which are near them, called "faculæ," but also of the general uneven groundwork of the sun's disk.

In tracing the path of a "spot" across the disk of the sun, I employ a Kellner (positive) eyepiece of about 50 linear, on which are engraved five transit-lines, at intervals of about $\frac{10}{117}$ of the sun's diameter, and therefore, near the centre of the disk, about equal to 10°. Having, with this eyepiece applied to the diagonal sun-glass, and the clock movement, determined the position of the spots, I replace the Kellner by ordinary (negative) eyepieces, whose powers range from 75 to 300, the latter being seldom beneficial except for objects near the limb, and in very favourable weather—which, in my experience, means a partially clouded sky, westerly winds, and morning hours by preference. Some of the clearest views which I have experienced were had in the intervals of storms and snow-clouds—the slowly drifting snow-flakes of those clouds being visible as dark spots in the field of view, when the eyepiece was adjusted to their very moderate distance *.

I present a diagram† showing the appearance of the sun's disk on the 29th of March, 1864, and other sketches of the bright and shady parts of the surface. From the variations in the appearance of the spots, faculæ, and ground surface, many suggestions arise; but I limit myself on this occasion to some inferences which appear justified by the observations of the least variable of them. So great is the diversity in the short

* I have occasionally employed the telescope to measure the distances and heights of clouds, employing for the purpose a well-known formula.

† Of the drawings accompanying this paper, which are referred to by numbers, Nos. 3, 4, and 5 are given in Plate III.; the rest are preserved in the Archives.

history of these ephemeral phenomena, that no view of their origin and progress can be held to be so well established at present as to exclude the consideration of other speculations.

Faculæ.—Near the edge of the solar disk, and especially about spots approaching the edge, it is quite easy, even with a small telescope, to discern certain very bright streaks of diversified form, quite distinct in outline, and either entirely separate, or coalescing in various ways into ridges and network. When, near the limb, the spots become invisible, the undulated shining ridges and folds to which I refer still indicate their place—being more remarkable thereabout than elsewhere on the limb, though everywhere traceable in good observing-weather* (see Plate III.). Faculæ are the most brilliant parts of the sun; they appear of all magnitudes, from barely discernible softly gleaming narrow tracts 1000 miles long to continuous complicated and heapy ridges 40,000 miles and more in length, and 1000 to 4000 miles broad. By the frequent meeting of the bright ridges, spaces of the sun's surface are included of various magnitudes and forms, somewhat corresponding to the areas and forms of the irregular spots with penumbræ. They are never regularly arched, and never formed in straight bands, but always devious and minutely undulated, like clouds in the evening sky, or irregular ranges of snowy mountains. When carefully studied with powers of 75, 135, and 180, which are very effective, the ridges appear prominent into cusps, and depressed into hollows; the cusps having brighter and more shady sides, so as not to be unlike some forms of branching crystallized native silver.

Ridges of this kind often surround a spot, which appears the more conspicuous from the surrounding brightness; but sometimes there appears a very broad white platform round the spot, and from this the white crumpled ridges pass in various directions (see Diagram, October 16, 1862, No. 2). Toward the limb the ridges appear parallel to it; further from it, this character is exchanged for indeterminate direction and lessened distinctness; over the remainder of the surface they are much less conspicuous, but can certainly be traced as an irregular network, more or less disguised by the minuter structure which has been described as porosity. I present selected sketches of the appearances mentioned. In considering the faculæ with attention, I remark that they preserve their shapes and positions with no material change during a few hours of observation, and probably for much longer periods, since after rotation through 15° the main features appear much the same as before.

The faculæ look like half-shaded snowy mountains and like half-illuminated clouds, and one might suppose that in either of these cases their

* In a recent communication to the Royal Society (Proceedings, vol. xiii. p. 168), Mr. Balfour Stewart remarks that, in the photographs of the sun taken at Kew, it appears to be a "nearly universal law that the faculæ belonging" to a spot "appear on the left of that spot, the motion due to the sun's motion of rotation being across the picture from left to right." I find that my sketches support this view to the extent that the faculæ which follow a spot appear in several cases more prominent than in others. Perhaps in a photograph those only can be traced, or the differences may seem to be greater.

elevated parts should project beyond the general circular outline. This I have never seen to happen; and if a little attention be given to proportions, it will appear very unlikely to be often observed. If the breadth of one of the ridges be taken at 4000 miles, its average slope 30°, and its height at a quarter of the breadth, viz. 1000 miles, this, if projected beyond the circular disk, would no doubt be discernible, for it would be equal to $\frac{1}{850}$th of the sun's diameter, or above 2″. But the number of the ridges is so great, and the crowding of them very near and on the limb so close, that it is hardly possible, on the average, for any ridge to be seen much above its fellows, whether the faculæ be in the atmosphere or in the body of the sun.

The continuous circularity of the sun's limb is therefore no sufficient argument in itself against the faculæ being much raised above the general surface; and it seems worth while to make special researches as to the manner in which faculæ come on and go off, in the hope of finding some case which shall be decisive.

General Ground.—In examining the interspaces between the faculæ in the parts towards the limb, it is not easy to see more on the surface thus inclined to the visual ray than a certain unevenness of tint; under a higher angle of incidence, toward the interior of the disk, this ground acquires more evident partial shades and lights—a sort of granulation difficult to analyze, especially if, as is most frequent, the edge of the sun shakes with varying refraction. If all be quite steady, however, the eye discerns both the ramifications of faculæ and the granulation of the interspaces, and by degrees acquires the power of seeing this easily and clearly. Under this condition, the perpetual motion and inconstant agitation, so often described, by no means appears to be a characteristic phenomenon, but rather as an exceptional trouble to the observer, arising from inconstant refraction, fatigue of the eye, or shake of the instrument. These difficulties increase rapidly with high powers; but with a power of 100, and good weather, the sun's face does not appear to be much obscured by this kind of intestine agitation.

What is seen appears in the diagram for April 2, 1864, as well as I can represent a structure so minute and perplexing. The ground of the whole is a surface of complicated small lights and shades, the limits of which appear arched or straight or confused according to the case; and the indeterminate union of these produces sometimes faint luminous ridges, the intervals filled up by shaded interstices and insulated patches of illuminated surface. One eminent observer *, seeing these under a high power, has compared them to willow leaves, unarranged except where they conform in some degree to the great features of the spots. Using the same kind of analogy, one might say they seem to resemble any somewhat uneven surface composed of separate masses, presenting themselves in all directions and at different levels. Being of all shapes, they must generally have one transverse measure greater than the other, and thus

* Mr. Nasmyth.

appear for the most part oblong, but with no systematic concurrence of direction. The comparison used by another astronomer *, of an irregularly heaped surface of rice, here collected into ridges, there sunk into depressions which seem to be occasionally deepened into pits, has its advantages. But upon the whole I prefer to be content with the less definite analogy to an irregular granulated and pitted surface, composed of small prominent lights complicated with intermediate shades of different intensities.

Part II.—*Details of Spots.*

In the midst of the general ground thus described, spots make their appearance, grow, change, and decay under the observer's eyes. Seldom indeed, as once to Mr. Carrington, does it happen that a spot appears suddenly; its growth, change, and decay are all usually slow—too slow to be positively noted and measured except at intervals of hours and days. We notice occasionally spots of minute size which have no surrounding penumbra; these may be either very black or only dark. These often occur in considerable numbers near a large spot well enclosed by penumbra, as in spot No. 2, 29th March (Drawing No. 7). On the 30th of March one of these had acquired a slight penumbra, several of the others had disappeared. The several small spots represented (Diagr. No. 6) amidst a mottled surface of faculæ and granulations had no penumbra; one was dark, but not black. Some of the spots here referred to appear to be less than 500 miles across, and many are less than 1000. They are occasionally in twins, and not unfrequently disposed in groups which may eventuate in a spot, or in a long sinuous tract having some relation of origin to greater spots near them. The greater spots with large penumbral regions have usually very irregular boundaries, and equally irregular black nuclei. In them it is quite usual to perceive two, three, or more nuclei; but in such cases the term nucleus is but little fitted to describe these often narrow winding and branching spots which appear within the irregular space. The boundaries of that space are in a certain degree conformed to the black nuclei. Many large spots of this kind undergo very considerable changes in the course of one day. A complicated spot which measured about 20,000 miles across on the 29th of March, lost its penumbra and was reduced to separate nuclei on the 31st; while a curved collection of small dots near it was concentrated to a small group, and two other obscure dots expanded a little and became three. In this state, with but slight further change, they all passed across 60° of arc.

In complete contrast with these examples is the spot No. 4 [in the diagram of March 29], which with very little observable change in itself, and with no additional productions near it, has been traced across the sun's disk through a third of the circumference. I present drawings of this spot as it appeared with powers of 75 and 135, for the purpose of marking the unusual persistency of its characters, and describing its relations to the

* Mr. Stone.

surrounding tracts of the sun's surface. It will be observed that the nucleus is black, not quite uniformly so, however, but with some inequality of tint; and that it is branched or notched at the edges, in this respect exhibiting *some* differences at different dates, which I do not think are due to any error or uncertainty in the sketches. The ring space round the spot, though not quite uniform, presented no inequalities that I could mark; the border was grey, striated, and unevenly extended into the surrounding tracts. These unevennesses appeared to me to be little varied, during several days. The striations and extensions in a radiating direction correspond to what Mr. Dawes calls by the picturesque name of "thatch"—a name singularly appropriate, if this border overhangs, as is often supposed, a cavity or depression. This "thatch," according to Mr. Nasmyth, is formed by the concurrent outlines of his "willow leaves"; but Professor Airy, in commenting on Mr. Stone's recent communication to the Royal Astronomical Society, is reported to have called attention to the fact that the rice-like aggregations were "quite distinct from the thatching so graphically described by Mr. Dawes in the penumbræ"*.

Until the opportunity shall have occurred to me of observing and drawing the edges of penumbræ and nuclei under a sufficient variety of circumstances, I must not offer to reconcile these apparently different opinions; at present my impression is that the "thatched" edges of the penumbra are only broken parts of the general groundwork of the body of the sun; which may be, or rather must be supposed to be very unequally luminous in different parts, the depressed and granulated parts emitting the least light.

The spot No. 4 on the Diagram for March 29 was first seen near the edge of the sun, surrounded by faculæ of great brilliancy †. This spot had the elliptical outline due to its position; the black nucleus was central; the border was shaded and striated; between the border and the nucleus was a clear bright space, not then appearing grey or dusky. As the spot moved on toward the central part of the disk, the oval became less and less eccentric; the nucleus remained central, and the border retained its dusky and striated aspect; but the ring round the nucleus lost its brightness, and took up more of the grey tint which belongs to the general surface of the sun when a very dark glass is employed ‡.

If these circumstances be well considered, they appear sufficient to prove that the spot taken as a whole is not sunk very much below, or raised very much above, the general level of the region. For were its interior part sunk very much below the border, it would have presented to the eye in passing from near the edge toward the centre the appearances sketched in

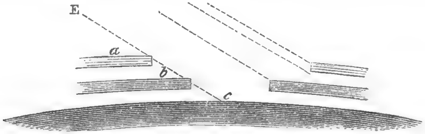
* 'Reader,' 2nd April, 1864.

† The drawings are made with the solar eyepiece, placed as in a Newtonian reflector, on the western side of the telescope. They do not, therefore, correspond with drawings or photographs taken by the ordinary arrangement without diagonal reflector.

‡ The full opening of the telescope (6 inches) being employed for the sake of exact definition, a very dark glass is required if the sun be clear.

the annexed diagram, not those which really occurred, as shown in the diagram already referred to.

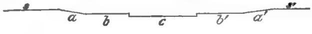
If we regard the spot as marking an opening in a bright photosphere, through which the dark body of the sun is seen within a terraced penumbra, as represented in the diagram of Sir J. Herschel* (here copied), it is obvious



that toward the edge of the sun, when the line of sight, E *c*, forms a very small angle with the surface of the sun, one side of the supposed terrace *b* will become invisible if the intervals of height between *b* and *a* be very considerable, and in the same way a part or the whole of *c* will be lost. The photosphere of the sun, then, if it exist, appears not to have the enormous depth sometimes ascribed to it; or if we suppose the spot, with its ring and border, to be a terraced concavity in the solid globe of the sun as under, with very steep sides, and breadths, $a=1$, $b=2$, $c=3$, the whole

from *a* to *a′* being 13,500 miles, it will be obvious that the difference of the levels of *a* and *b* cannot have so great a proportion to the sun's diameter as the crater-walls of the moon bear to the diameter of that satellite, or else cannot have anything like their steepness. The moon's crater-walls are in height $\frac{1}{2000}$ or even $\frac{1}{1000}$th of the diameter, and often very steep. On the sun the same proportion would give cliffs 400 or 800 miles high, and with the spot in a position 60° from the centre of the sun, such cliffs would on one side conceal half or more of one side of the terrace (*b*), while the other side (*b′*) of the terrace remained entirely visible.

If we suppose that *a* is not a steep cliff but a prolonged slope, so that even toward the edge of the sun the whole of the interior area may be seen, the limit of the difference of level between the general surface (*s*) and the interior terrace (*b b′*) can be calculated. For example,



In former observations of remarkable spots (1862), and again in 1863

* Outlines of Astronomy (Ed. 1833), pl. 3. *d.*

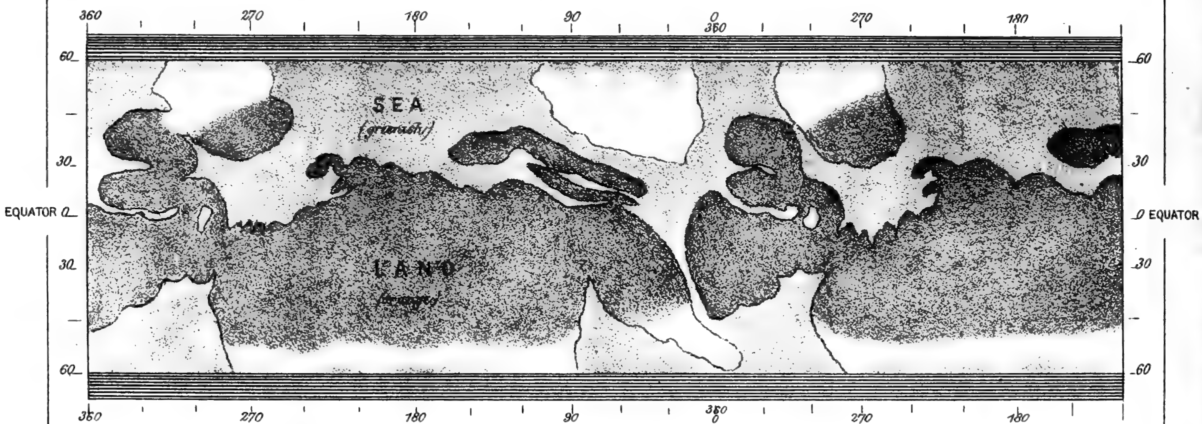
and 1864, I have several times noticed this persistence of the elliptically contracted spot with its nucleus, equidistant from the borders, very near to the edge, both coming on and going off, certainly within 10° of the edge, from which it may be inferred that in that case the angle of inclination of the edge of the spot to the general surface could not be greater than 10°. Taking the case of 10°, and applying it to the spot now under consideration, the difference of level in miles between *s*, the general solar surface, and *b*, the ring terrace, might $=a \sin 10°=300$ miles, but could not exceed it. This result is represented in the preceding diagram.

Nor can the spot be sunk in a deep saucer-shaped concavity like

for the same reasons.

But, however deeply the spot may be sunk below the surface of the sun, no notch could appear in consequence of that at the limit of the sun; for before reaching the limb the angle $a\,\mathrm{E}\,a'$, under which it is seen from the earth (E), would become invisibly small, and the space $a-a'$ become invisible. Even if the sides a and a' were very unequal in level, the leading edge a' being depressed, this would make no visible notch on the limb of the sun, except the spot were enormously large, as well as very deep—much greater for instance than 40,000 miles. Except in very rare cases, then, the sun's edge must always appear truly circular, notwithstanding the depressions of the spots and the elevations of the faculæ.

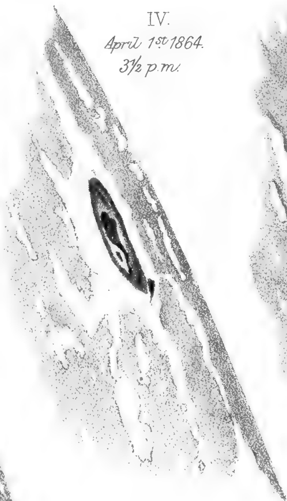
Finally, I remark that the spots may appear black, dark, grey, &c., not because they really are so dark as they seem, but that, being less luminous than other parts of the disk, they acquire this relative darkness under the operation of the optical apparatus, and the influence of contrast on the sensation. An extremely good way of viewing the spots is to project the sun's image on to a smooth porcelain screen, about a foot or 18 inches in diameter; very smooth white paper answers very well. Thus tried, every imaginable degree of relative darkness appears in the spots, and the faculæ come out bright and distinct. In this experiment, the spots seem so dark in the nuclei as to suggest the hypothesis that the parts of the sun to which they correspond really emit specially heat-rays, below the range of refrangibility which brings to our eyes light and the power of sight. Heat-rays and light-rays come to the earth together, but that is no reason for thinking they must spring in mixed pencils from every part of the sun equally. In my way of considering it, this rather confirms the idea of the deep black nuclei being the sun's body, the penumbræ that body partially seen through the atmosphere, and the facular region transmitting to us rays which have acquired a higher refrangibility than that with which they started, by a peculiar change in the sun's atmosphere, which may justly be called his photosphere.



MARS. — 1862. 1864.

Equatorial Projection by Professor Phillips.

J. Basire lith.



Faculæ. Spot approaching limb. Spot nearing the limb. Spot disappearing.

February 2, 1865.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "On a New Geometry of Space." By JULIUS PLÜCKER, For. Memb. R.S. Received December 22, 1864.

(Abstract.)

Infinite space may be considered either as consisting of points or transversed by planes. The points, in the first conception, are determined by their coordinates, by x, y, z for instance, taken in the ordinary signification; the planes, in the second conception, are determined in an analogous way by their coordinates, introduced by myself into analytical geometry, by t, u, v for instance. The equation

$$tx+uy+vz=1$$

represents, in regarding x, y, z as variable, t, u, v as constant, a plane by means of its points. The three constants t, u, v are the coordinates of this plane. The same equation, in regarding t, u, v as variable, x, y, z as constant, represents a point by means of planes passing through it. The three constants x, y, z are the coordinates of this point.

The geometrical constitution of space, referred hitherto either to points or to planes, may as well be referred to *right lines*. According to the double definition of such lines, there occurs to us a double construction of space. In the first construction we imagine infinite space to be traversed by lines, themselves consisting of points; an infinite number of such lines in all directions pass through any given point; the point may describe each of the lines. This constitution of space is admitted when, in optics, we regard luminous points sending out in all directions rays of light, or, in mechanics, forces acting on points in any direction. In the second construction, infinite space is regarded likewise as traversed by right lines, but these lines are determined by planes passing through them. Every plane contains an infinite number of lines having within it every position and direction, round each of which the plane may turn. We refer to this second construction when, in optics, we regard, instead of rays, the corresponding fronts of waves and their consecutive intersections, or when, in mechanics, according to Poinsot's ingenious philosophical views, we introduce into its fundamental principles "couples," as well entitled to occupy their place as ordinary forces. The instantaneous axes of rotation are right lines of the second description.

The position of a right line depends upon four constants, which may be determined in a different way. I adopted for this purpose the ordinary system of three axes of coordinates. A line of the first description, which

we shall distinguish by the name of *ray*, may be determined by means of two projections, for instance by those within XZ and YZ, represented by

$$x = rz + \rho,$$
$$y = sz + \sigma,$$

or by

$$tx + v_x z = 1,$$
$$uy + v_y z = 1.$$

In admitting the first system of equations, a ray is determined in a linear way by means of the four constants r, s, ρ, σ, which may be called its four coordinates, two of them, r and s, indicating its direction, the remaining two, after its direction being determined, its position in space. In adopting the second pair of equations, t, u, v_x, v_y will be the coordinates of the ray.

A right line of the second description, which we shall distinguish by the name of *axis*, is determined by any two of its points. It is the common intersection of all planes passing through both points. We may select the intersection of the axis with the two planes, XZ and YZ, as two such points, and represent them by

$$xt + z_t v = 1,$$
$$yu + z_u v = 1,$$

or by

$$t = pv + \pi,$$
$$u = qv + \kappa.$$

In making use of the first pair of equations, the four constants x, y, z_t, z_u, indicating the position of the two points within XZ and YZ, are the coordinates of the axis. In adopting the second pair, the four coordinates of the axis are p, q, π, κ.

A complex of rays or axes is represented by means of a single equation between their four coordinates; a *congruency*, containing all congruent lines of two complexes, by means of two such equations; a *configuration*, containing the right lines common to three complexes, by three equations. In a complex every point is the vertex of a cone, every plane contains an enveloped curve. In a congruency there is a certain number of right lines passing as well through a given point as confined within a given plane. A configuration is generated by a moving right line.

In a *linear complex* the right lines passing through a given point constitute a plane; all right lines within a given plane pass through a fixed point. Two linear complexes intersect each other along a *linear congruency*. In such a linear congruency there is a single right line passing as well through a given point as confined within a given plane. Three linear complexes meet along a *linear configuration*.

Instances of linear complexes are obtained by means of linear equations between the four coordinates of any one of the four systems. A linear configuration of rays represented by three such equations between r, s, ρ, σ is a paraboloid, immediately obtained; between t, u, v_x, v_y a hyperboloid.

A linear configuration of axes represented by three linear equations between p, q, π, κ is a hyperboloid, immediately obtained; between x, y, z_t, z_u a paraboloid. Instances of linear congruencies are exhibited by means of two linear equations, as well between t, u, v_x, v_y as between x, y, z_t, z_u, and their right lines easily constructed.

The general linear equation, however, between any four coordinates does not represent a linear complex of the most general description. Besides, there is a want of symmetry, the four coordinates depending upon the choice of both planes, XZ and YZ. This double inconvenience, if not eliminated, would render it impossible to adapt in a proper way analysis to the new geometrical conception of space. But it may be eliminated in the most satisfactory way.

For that purpose I introduced (in confining myself to the case of the coordinates r, s, ρ, σ) a fifth coordinate $(s\rho - r\sigma)$, which is a function of the four primitive ones. Then the linear equation between the five coordinates

$$Ar + Bs + C + D\sigma + E\rho + F(s\rho - r\sigma) = 0$$

is the most general of a linear complex. After having been rendered homogeneous by a sixth variable introduced, it becomes of a complete symmetry with regard to the three axes OX, OY, OZ. The introduction of the fifth coordinate $(s\rho - r\sigma)$ is the real basis of the new analytical geometry, the exploration of which is indicated in the ordinary way.

In the paper presented, a complete analytical discussion of a linear complex is given. We may for any point of space construct the *corresponding plane* containing all traversing rays, and *vice versâ*. Right lines of space associate themselves into couples of *conjugated lines*; to each line a conjugated one corresponds. Any right line intersecting any two conjugated, is a ray of the complex. Each ray of it is to be regarded as two coincident conjugated lines. It is easily shown that each linear complex may be represented by means of any one of the following three equations, in which k indicates the same constant:

$$s\rho - r\sigma = k, \quad \sigma = kr, \quad \rho = ks.$$

Accordingly a linear complex depends upon the position of a fixed line (depending itself upon four constants) and the constant k. Hence it likewise follows that such a complex of rays may, without being changed, as well turn round that fixed line, *the axis of the complex*, as move along it, parallel to itself. The same results may be confirmed by means of the transformation of ray-coordinates, and thus analytically determined by the primitive constants A, B, C, D, E, F, the position of the axis of the complex and its constant k. In a peculiar case, where k becomes *zero*, all rays of the complex meet its axis.

A linear congruency of rays, along which an infinite number of linear complexes meet, is represented by the equations of any two of these complexes. Through a given point of space passes only one ray, *corresponding* to it, as there is only one corresponding ray confined within a given

plane. There is, with regard to each complex passing through the congruency, one right line conjugated to a given one. All these conjugated lines constitute one generation of a hyperboloid, while the right lines of its other generation are rays of the congruency, which therefore may be generated by a variable hyperboloid turning round one of its right lines.

The axes of all complexes intersecting each other along a linear congruency meet at right angles a fixed line, which is the axis of the congruency. Among the complexes there are especially two, the axes of which are met by their rays. These axes, meeting themselves the axis of the congruency, are its directrices. A linear congruency, depending upon eight constants, is fully determined by means of its two *directrices*. Each right line intersecting both directrices is one of its rays. The plane parallel to both directrices, and at equal distance from them, is the *central plane* of the congruency; the point where it meets, under right angles, the axis of the congruency, its *centre*. The two lines bisecting within the central plane the projections of the two directrices, are its *secondary axes*. The directrices may be as well both *real* as both *imaginary*. In peculiar cases the two directrices are congruent, or one of them is at an infinite distance. Each of two complexes being given by means of its axis and its constant k, both directrices of the congruency along which they intersect one another are analytically determined. A congruency being given by means of its directrices, the constants and axes of all complexes passing through it are determined.

A *linear configuration of rays* is the common intersection of any three linear complexes, and represented by their equations, $\Omega=0$, $\Omega'=0$, $\Omega''=0$. Each complex represented by an equation of the form $\Omega+\mu\Omega'+\nu\Omega'=0$, equally passes through the same configuration. So does any congruency along which two such complexes meet. A linear configuration is a hyperboloid; its rays constitute one of its generations, while the directrices of all traversing congruencies constitute the other. The central planes of all these congruencies meet in the same point—the *centre* of the hyperboloid. Its *diameters* meet both directrices of the different congruencies. The directrices are either real or imaginary; accordingly the diameters meet the hyperboloid, or meet it not. If the two directrices are congruent, the diameters become asymptotes. The hyperboloid passes into a paraboloid if there is one directrix infinitely distant.

A linear configuration is determined by means of three congruencies as it is by means of three complexes. That ray of it which meets one directrix of each congruency is parallel to the other. By drawing two planes through the two directrices of each of the three congruencies parallel to its central plane, we get a rhomboid circumscribed about the hyperboloid, the points of contact, within the six planes, being the points where the six directrices are intersected by the rays. A hyperboloid being given, we may revert to the congruencies and complexes constituting it. Finally, the equation of the hyperboloid in ordinary coordinates, x, y, z, is derived.

If we proceed to complexes of the second degree, the field of inquiry is immensely increased. Here any given point of infinite space is the vertex of a cone of the second order, and likewise within any given plane there is a curve of the second class enveloped by rays of the complex. The whole of the infinite number of cones, as well as of the infinite number of enveloped conics, is represented by a linear equation, between the five ray-coordinates r, s, ρ, σ and $(s\rho - r\sigma)$. The general analytical theory of contact may immediately be applied to complexes of the second order, touched by linear complexes, &c.

In order to elucidate the geometrical conceptions explained, I thought it proper to present, in Section II., an application to optics, leading to a complex of a simple description. Rays of light, constituting in air a complex, will likewise do so after being submitted to any reflexions or refractions whatever. Let us, for instance, suppose that the complex in air is of the first order and its constant equal to zero; *i. e.*, that its rays start in every direction from all points of a luminous right line. Let these rays enter a *biaxal crystal* by any plane surface. Let the luminous line and this surface be perpendicular to each other. Then, within the crystal, the double-refracted rays constitute a new complex, which is represented, like the primitive one, and independently of it, by means of an equation between ray-coordinates.

For this purpose I return to a paper of mine of the year 1838, concerning double refraction, at the end of which, after having mentioned the application of Huyghens's principle to Fresnel's wave-surface and the construction of Sir William Hamilton, I proposed a new construction of the double-refracted rays in the most general case. Here I first made use of an auxiliary ellipsoid, with regard to which the polar plane of every point of the wave-surface is one of its tangent planes, and, reciprocally, the pole of every plane touching the surface one of its points. In representing Fresnel's ellipsoid by the equation

$$a^2 x^2 + b^2 y^2 + c^2 z^2 = 1,$$

the new auxiliary ellipsoid may be represented by

$$\frac{x^2}{bc} + \frac{y^2}{ac} + \frac{z^2}{ab} = 1,$$

or

$$ax^2 + by^2 + cz^2 = abc,$$

and replaced, for most purposes, by the similar one,

$$ax^2 + by^2 + cz^2 = 1.$$

The construction, as far as we are concerned here, may be expressed thus: —Construct at the moment when Fresnel's wave-surface is formed the polar line of the trace along which the surface of the crystal is intersected by the elementary wave. The two refracted rays meet the wave-surface in the two points where it is intersected by the polar line constructed. In the paper of

1838 I promised a discussion of the construction given, but neglected it till the present time. This discussion immediately leads us to represent the complex of double-refracted rays by an equation, and at the same time we meet with several theorems worthy of notice.

If there is any incident ray, *the plane of refraction*, containing both double-refracted rays, is congruent with the diametral plane of the auxiliary ellipsoid, the conjugated diameter of which is perpendicular to the plane of incidence. All rays incident within the same plane are, after double refraction, confined again within the same plane. While the plane of incidence turns round the vertical, the corresponding plane of refraction turns round that diameter of the auxiliary ellipsoid, the conjugated diametral plane of which is the surface of the crystal. Whatever may be the plane or curved surface by which a crystal is bounded in a given point, all corresponding planes of refraction pass through a fixed right line.

A complex of rays starting in air in all directions from every point of a luminous right line, perpendicular to the surface of the crystal, is represented by the equation

$$r\sigma = s\rho,$$

the luminous right line being the axis OZ, while the two remaining axes, OX and OY, are within the surface of the crystal any two right lines perpendicular to each other. This complex is transformed by double refraction into another, the equation of which assumes the most simple form,

$$r\sigma = ks\rho,$$

in especially admitting that the two axes, OX and OY, are congruent with the axes of the ellipse along which the auxiliary ellipsoid is cut by the surface of the crystal, and that the third axis, OZ, is, within the crystal, the diameter of the auxiliary ellipsoid; the conjugated diametral plane is that surface. k is a constant indicating the ratio of the squares of the two axes of the ellipse.

The complex of double-refracted rays is of the second order; its equation may be easily submitted to analytical discussion. All its rays passing through any given point constitute a cone of the second order. This cone remains the same if the point describes a right line, passing through the origin. Likewise there is in any given plane a hyperbola, enveloped by rays of the complex. Peculiar cases are easily determined. The complex of double-refracted rays may be described in three different ways by a variable linear congruency. In the peculiar case in which the surface of the crystal is a principal section, OZ becomes perpendicular to it; if it is one of the circular sections of the auxiliary ellipsoid, the constant k becomes equal to unity, *i. e.* all double-refracted rays meet the axis OZ. From the general case the case of uniaxal crystals is immediately derived.

II. "Researches on Solar Physics.—Second Series. On the Behaviour of Sun-spots with regard to Increase and Diminution." By WARREN DE LA RUE, Ph.D., F.R.S., President of the Royal Astronomical Society, BALFOUR STEWART, A.M., F.R.S., Superintendent of the Kew Observatory, and BENJAMIN LOEWY, Esq. Received January 10, 1865.

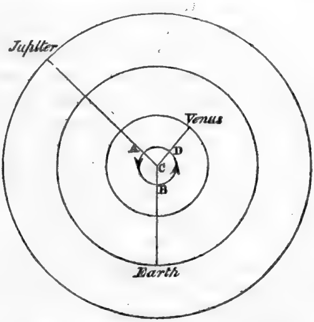
(Abstract.)

One of the authors of this paper having been led, from a preliminary investigation, to suspect that the behaviour of sun-spots with respect to increase and diminution refers to some extraneous influence, they resolved to investigate the behaviour in this respect of the spots observed by Carrington, in addition to the Kew photograms up to the present date.

The authors have thus examined materials embracing a period of ten years, and in this paper state the result.

The nature of their examination is thus described :—

If we imagine great circles of ecliptical longitude to be drawn from the sun's centre, every point of the sun's surface as it moves round by rotation will of course pass successively through each of those great circles, and every one of the planets will do the same as they move round by their own proper motions.



And if we imagine the plane of the paper to denote the plane of the ecliptic, and project upon this plane each body of our system, we shall have a scheme similar to the above, in which ADB, the inner circle, may represent the sun himself, the next circle, let us say the orbit of Venus, the next that of our earth, while the outer may denote the orbit of Jupiter. To an observer looking down upon our system from the north, all motions will be in the direction of the arrow-heads, that is to say, in a direction contrary to that of the hands of a watch, or left-handed, while ecliptical circles of

longitude will be represented by the various radii proceeding from the centre C, the angular difference between the two radii denoting the angular difference between the two corresponding longitudes. If the observer be stationed at the earth, all points of the solar surface will advance by rotation from left to right across the visible disk; while the radii vectores of the inferior planets Mercury and Venus, which move faster than the earth, will appear to the terrestrial observer to have a left-handed rotation, in such a manner that the planet Venus will move from its place in the diagram to opposition, and ultimately come round to conjunction from the *left*. On the other hand, the superior planets, which move more slowly than the earth, will appear to the terrestrial observer to have a right-handed rotation, in such a manner that Jupiter will proceed from his place in the diagram to opposition, and ultimately come round in conjunction by the *right*. Also the point B, which occupies the central position of the visible solar disk, will have the same heliocentric longitude as the earth. Let us make the central longitude, or longitude corresponding to the position of the earth at the time of observation, our meridian, and let us reckon as negative all longitudes less than 180° to the left, and as positive all those less than 180° to the right. In this way a spot or point of the sun's disk, as it comes round by the left limb, will have the longitude -90, while, as it disappears by the right limb, its longitude will be $+90$. Hence also the longitude of Jupiter in the diagram will be $-$ACB, while that of Venus will be $+$BCD. If the angle ACB is very large, we may say that Jupiter is much to the left, and if BCD is large, we may say that Venus is much to the right. In the examination to which the spots have been subjected, it has been endeavoured to ascertain, as nearly as possible, at what longitude any spot breaks out, or at what longitude it reaches its maximum and begins to wane. Very often, however, we are not able to assign the exact longitude of such an occurrence; but yet, as will be seen in the sequel, we are able to determine, in a general way, the behaviour of spots.

The examination was made in the following manner. Mr. Carrington's original drawings were examined by two observers noting the behaviour of each spot, and the results again compared with Carrington's published maps, which give the behaviour of spots from day to day; ultimately a list was obtained, no spot available for comparison being left out. A similar process was followed with regard to the Kew pictures.

It is to be remarked, that in making the examinations of the Carrington pictures, both observers were ignorant of the planetary configurations; and that although with regard to the Kew pictures one observer knew the corresponding planetary configurations, yet his judgment, being checked by his fellow-observer, could not be biassed by any previous speculative views.

In a Table given, showing the behaviour of sun-spots from the beginning of 1854 to the end of 1864, it is seen that different spots occurring about the

same time on the sun's disk behave themselves in the same manner; so that if one spot, after making its appearance, increases until the centre line, another will do the same; or if one spot breaks out on the left or on the right, half the other spots about the same period have a tendency to break out on the same half. Examples of these are referred to in the Tables.

The authors suppose that this peculiarity of behaviour of spots can only be explained by reference to some influence from without. Suppose that such an influence, of a nature unfavourable to spot-production, exists, then, as spots are brought round to it by rotation, they will gradually wane; and, on the other hand, as the surface departs from it, spots will break out. But while there is good evidence for believing in the existence of some such influence, it is a very difficult thing to determine its nature, and one which can only be done very imperfectly with our present knowledge.

The authors attempt to answer the following questions. Is this influence stationary? or, if moveable, can it be traced to any of the planets of our system?

The behaviour of each series of groups is then compared with the positions of the three planets, Mercury, Venus, and Jupiter, at the same date; these planets being imagined to be the most influential; since the first, though small, is very near the sun, the second is both near and tolerably large, while the last, although distant, is of very great mass.

In answer to the first question, Is the influence stationary? it may be remarked that if it be so, the difference of behaviour noticed at different periods must be due to the position of the earth, or point of view at these periods with reference to the stationary influence, and hence in similar months of different years we should have a similar behaviour; but it cannot be found from investigations that there is any connexion between a certain behaviour of sun-spots and a certain period of the year, and hence there is no reason to suppose that the external influence is fixed.

In the next place, does this influence, if moveable, move faster or slower than the earth? If faster, it will proceed from conjunction to opposition, passing over the sun's disk from left to right. If we view it as one unfavourable to the production of spots, then, at first, when it is near conjunction, or a little to the left, the sun's surface to the right, receding from it, will break out into spots; but as the influence moves on to the right, spots will come towards it from their first appearance, and will consequently decrease from the first. But if, on the other hand, the influence move more slowly than the earth, it will move from conjunction to opposition, from right to left; so that a tendency of spots to form on the disk will be followed by a tendency to *increase*, not *decrease*, after making their appearance.

The order of the consecutive phenomena will thus be different in the two cases.

It is shown by a Table that a tendency of spots to break out is followed by a tendency of spots to decrease after making their appearance, and it is thereby concluded that the influence moves faster than the earth. This would seem to point to either Mercury or Venus as the agent in this matter, but the behaviour varies too slowly to be caused by the former. Venus, therefore, appears to be the influencing agent; and whether the behaviour of spots appears to depend on the position of this planet with reference to the earth, or point of view, the following Table, in which the spot-behaviour is compared with the corresponding position of Venus, will show:—

No. of series.	Behaviour.	Position of Venus.
1.	Increase to centre.	A good deal to left.
2.	Break out.	Conjunction.
3.	Decrease.	To right.
5.	Increase.	Near opposition.
6.	Break out.	Near conjunction.
...	....	....
7.	Increase.	Opposition.
8.	Break out.	Near conjunction.
9.	Uncertain behaviour.	To right.
11.	Increase to centre.	Near opposition.
12.	Increase past centre.	Near opposition (to the left).
13.	Break out.	Near conjunction.
15.	Decrease.	To right.
17.	Increase to centre.	Near opposition.
18.	Increase past centre.	Near opposition (to the left).
20.	Break out.	Near conjunction.
22.	Stationary behaviour.	To right.
24.	Increase to centre.	Near opposition.
25.	Break out.	Near conjunction.
*26.	Uncertain behaviour.	To right.
27.	Increase to centre.	Near opposition.
28.	Increase past centre.	Near opposition (to the left).
29.	Break out.	Near conjunction.
31.	Decrease shortly after appearance.	To the right.
32.	Increase to centre.	Near opposition.

It will be seen from this Table that the behaviour of spots appears to be connected with the position of Venus in such a manner that spots dissolve when that part of the sun's surface in which they exist approaches the neighbourhood of this planet; while, on the other hand, as the sun's disk recedes from this planet, spots begin to break out and reach their maximum on the opposite side.

* Venus and Jupiter are here opposed to one another.

There are a few cases in which Venus and Jupiter are opposed to one another; the authors do not, however, suppose that these instances are sufficient to prove the fact of an action due to Jupiter, but think it right, in alluding to them, to state at the same time the opposed position of the two planets, since this may furnish a possible explanation of the uncertain behaviour of spots by which these series are characterized.

The results of this paper may be stated briefly as follows:—

Observed fact.—Spots appearing about the same time on the sun's disk behave in the same manner as they pass from left to right.

Legitimate deduction.—The behaviour of spots is influenced by something from without, and from the nature of the spot-behaviour the authors conclude that this influence travels faster than the earth; and finally, they find that the behaviour of spots appears to be determined by the position of Venus in such a manner that a spot wanes as it approaches this planet by rotation, and, on the other hand, breaks out and increases as it recedes from the neighbourhood of the planet, reaching its maximum on the opposite side.

In conclusion, it is not meant in this paper to convey the idea that Venus is the cause of the ten-yearly period of sun-spots, but merely that there is a *varying behaviour* of spots which appears to have reference to the position of this planet, or, putting aside the influencing agent, appears to have reference to certain ecliptical longitudes.

III. "On the Rapidity of the Passage of *Crystalloid* Substances into the Vascular and Non-Vascular Textures of the Body." By Henry Bence Jones, F.R.S. In a Letter to the Secretary. Received February 2, 1865.

Dear Dr. Sharpey,—I am anxious that you should read to the Royal Society a short note containing the results of some observations I lately made on the rapidity of the passage of *crystalloid* substances into the vascular and non-vascular textures of the body.

It occurred to me that it might be possible to trace the passage of substances from the blood into the textures of the body by means of the spectrum-analysis, and with the assistance of Dr. Dupré some very remarkable results have been obtained.

Guinea-pigs have chiefly been used for the experiments. Usually no lithium can be found in any part of their bodies. When half a grain of chloride of lithium was given to a guinea-pig for three successive days, lithium appeared in every tissue of the body. Even in the non-vascular textures, as the cartilages, the cornea, the crystalline lens, lithium would be found.

Two animals of the same size and age were taken; one was given 3 grains of chloride of lithium, and it was killed in eight hours; another had no

lithium; it was also killed, and when the whole lens was burnt at once, no trace of lithium could be found. In the other, which had taken lithium, a piece of the lens, $\frac{1}{20}$th of a pin's head in size, showed the lithium; it had penetrated to the centre of the lens.

In another pig the same quantity of chloride of lithium was given, and in four hours even the centre of the lens contained lithium.

Another pig was given the same quantity, and it was killed in two hours and a quarter. The cartilage of the hip showed lithium faintly, but distinctly. The outer portions of the lens showed it slightly; the inner portions showed no trace.

To a younger pig the same quantity was given, and it was killed in thirty-two minutes. Lithium was found in the cartilage of the hip; in the aqueous humour; distinctly in the outer part of the lens, and very faintly in the inner part.

In an older and larger pig, to which the same quantity was given, lithium after one hour was found in the hip and knee joints very faintly; in the aqueous humour of the eye very distinctly; but none was found in the lens, not even when half was taken for one trial.

Chloride of rubidium in a three-grain dose was not satisfactorily detected anywhere. When 20 grains had been taken, the blood, liver, and kidney showed this substance; the lens when burnt all at once showed the smallest possible trace; the cartilages and aqueous humour showed none, probably because the delicacy of the spectrum-analysis for rubidium is very much less than that for lithium.

A patient who was suffering from diseased heart took some lithia-water containing 15 grains of citrate of lithia thirty-six hours before her death, and the same quantity six hours before death. The crystalline lens, the blood, and the cartilage of one joint were examined for lithium: in the cartilage it was found very distinctly; in the blood exceedingly faintly; and when the entire lens was taken, the faintest possible indications of lithium were obtained.

Another patient took lithia-water containing 10 grains of carbonate of lithia five hours and a half before death: the lens showed very faint traces of lithium when half the substance was taken for one examination; the cartilage showed lithium very distinctly.

I expect to be able to find lithium in the lens after operation for cataract, and in the umbilical cord after the birth of the fœtus.

I am, yours truly,

H. Bence Jones.

February 9, 1865.

Major-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, the Right Honourable Lord Dufferin was proposed for election and immediate ballot.

The proposal having been seconded, the ballot was taken, and Lord Dufferin was declared duly elected a Fellow of the Society.

The following communications were read :—

I. "Monthly Magnetical Observations taken at the College Observatory, Stonyhurst, in 1864." By the Rev. WALTER SIDGREAVES. Communicated, with a Note, by the President. Received January 24, 1865.

The Horizontal, Vertical, and Total forces are calculated to English measure, one foot, one second of mean solar time, and one grain being assumed as the units of space, of time, and of mass.

The Vertical and Total forces are obtained from the absolute measure of horizontal force and the Dip. The measures of the Dip-angle obtained with needle 2 have not been used in these calculations, as it appears from the observations taken with this needle that the position of its axle is less true than that of needle 1.

For the observations of Deflection and Vibration, taken each month for absolute measure of horizontal force, the same magnet has always been employed.

The moment of inertia of the magnet, with its stirrup, for different degrees of temperature, and the coefficients in the corrections required for the effects of temperature and of terrestrial magnetic induction on the magnetic moment of the magnet, were determined at the Kew Observatory by the late Mr. Welsh.

The moment of inertia of the magnet, with its stirrup, using the grain and foot as the units of mass and of linear measure, is 5·27303. Its rate of increase for increase of temperature is 0·00073 for every 10° of Fahr.

The weight of the magnet, with its stirrup, is approximately 825 grains, and the length of the magnet is nearly 3·94 inches. The moment of inertia was determined independently of the weight and dimensions, by the method of vibration with and without a known increase of the moment of inertia.

The temperature corrections have always been obtained from the formula $q(t_0-35^\circ)+q'(t_0-35)^2$, where t_0 is the observed temperature and 35° Fahr. the adopted standard temperature. The values of the coefficients q and q' are respectively 0·0001128 and 0·000000436.

The induction coefficient μ is 0·000244.

The correction for error of graduation of the Deflection bar at 1·0 foot is +0·00004 ft., at 1·3 foot +0·000064 foot.

The observed times of vibration are entered in the Table without corrections.

The time of one vibration has been obtained each month from the mean of twelve determinations of the time of 100 vibrations.

The angles of deflection are each the means of two observations.

In deducing from these observations the ratio and product of the magnetic moment m of the magnet, and the earth's horizontal magnetic inten-

sity X, the induction and temperature corrections have always been applied, and the observed time of vibration has been corrected for the effect of torsion of the suspending thread ; but no correction has been required for the rate of the chronometer, or for the arc of vibration, the former having been generally less than $1^{s}\cdot 0$, and the latter less than 50′.

A twist of the torsion circle through 90° has ordinarily deflected the magnet through 10′ of arc.

In the calculations of the ratio $\frac{m}{X}$, the third and subsequent terms of the series $1+\frac{P}{r^2}+\frac{Q}{r^4}+$ &c. have always been omitted. The value of the constant P was found to be $-0\cdot 00219$; this value being the mean of nine determinations obtained each from two pairs of deflection observations at

STONYHURST COLLEGE

Latitude 53° 50′ 40″. Longitude West of

TABLE of the Results of the Monthly

1864.	Abstract of Observations of Deflection and Vibration for absolute measure of Horizontal Force.									
Month.	Day and Hour.	Distance of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$.	Day and Hour.	Temperature.	Time of one vibration.	Log $m.X$.	Value of m.
	h m	foot.	°	° ′ ″		h m	°			
January ...	22nd... 2 0 p.m.	1·0	47·9	17 12 33	9·17175	21st ...11 30 a.m.	41·4	5·19981	0·28299	0·53379
February...	6th ... 1 0 p.m.	1·0	37·8	17 14 20	9·17183	6th ...11 0 a.m.	35·0	5·19158	0·28373	0·53429
March	12th ... 9 0 a.m.	1·0	41·4	17 13 42	9·17179	12th ... 2 0 p.m.	46·3	5·20921	0·28142	0·53286
April	11th ... 1 45 p.m.	1·0	55·7	17 3 29	9·16848	12th ... 6 0 p.m.	47·3	5·21762	0·27994	0·52995
	2 0 p.m.	1·3	56·0	7 40 55	9·16860	....	...	......	......	......
May	9th ... 5 45 p.m.	1·0	46·8	17 0 51	9·16688	9th ... 2 0 p.m.	57·7	5·22742	0·27933	0·52850
	„ 6 30 p.m.	1·3	45·5	7 39 47	9·16683	....	...	......	......	......
June.........	11th ... 6 0 p.m.	1·0	58·9	16 54 30	9·16508	11th ... 2 0 p.m.	63·5	5·24119	0·27766	0·52647
	„ 6 30 p.m.	1·3	58·4	7 36 59	9·16508	....	...	......	......	......
July	7th ... 6 15 p.m.	1·0	61·4	16 52 25	9·16439	7th ... 5 30 p.m.	69·0	5·23707	0·27827	0·52638
	„ 6 45 p.m.	1·3	60·0	7 36 1	9·16427	....	...	......	......	......
August......	6th ... 9 15 a.m.	1·0	61·0	16 52 44	9·16449	5th ... 5 0 p.m.	61·3	5·24643	0·27656	0·52550
	„ 9 45 a.m.	1·3	62·8	7 36 13	9·16466	....	...	......	......	......
September	10th ...10 15 a.m.	1·0	58·3	16 51 49	9·16392	10th ... 9 0 a.m.	62·3	5·25863	0·27463	0·52386
	„ 10 30 a.m.	1·3	58·9	7 35 28	9·16367	....	...	......	......	......
October ...	7th ...11 15 a.m.	1·0	54·7	16 48 55	9·16246	7th ...10 0 a.m.	54·5	5·25965	0·27371	0·52253
	„ 11 45 a.m.	1·3	55·3	7 34 33	9·16255	....	...	......	......	......
November	8th ... 2 50 p.m.	1·0	45·4	16 48 25	9·16162	8th ... 2 0 p.m.	48·3	5·25466	0·27427	0·52229
	„ 3 0 p.m.	1·3	44·5	7 34 12	9·16148	....	...	......	......	......
December	15th ...12 15 p.m.	1·0	39·9	16 47 28	9·16087	15th ...11 0 a.m.	40·0	5·26192	0·27260	0·52093
	„ 12 30 p.m.	1·3	38·8	7 34 6	9·16103	....	...	......	......	......

m represents the magnetic moment of the Deflecting Magnet.
X represents the Earth's Horizontal Magnetic Intensity.

distances 1·0 and 1·3 foot. The value of P obtained in the preceding year was $-0{\cdot}00217$.

The mean of the values of $\frac{m}{X}$, obtained each month from deflection observations at the two distances 1·0 and 1·3 foot, has been adopted for deducing the measure of horizontal force.

Observations taken to determine the position of the zero-point on the scale of the declination magnet, gave the same result as was obtained in 1859.

The discrepancies in the Declination observations may possibly be in a considerable degree occasioned by diurnal variation, as the observations varied in regard to the hour of the day; in future this will be avoided by making the observations always at a fixed hour. The discrepancies may also have been in part occasioned by magnetic disturbance which we have no present means of eliminating.

OBSERVATORY.

Greenwich $0^h\ 9^m\ 52^s{\cdot}68$. Height above Sea-level 381 feet.

Magnetic Observations for 1864.

Declination.		Magnetic Dip.			Absolute Measures. Values of			
Stonyhurst. Mean Time.	West Declination.	Day and Hour.	Needle.	Dip.	X, or Horizontal Force.	Y, or Vertical Force.	Φ, or Total Force.	Observer.
h m	° ′ ″	h m		° ′ ″				
29th ... 9 28 a.m.	21 52 35	21st ... 2 30 p.m.	1	69 47 21	3·5944	9·7634	10·4040	W.S
6th ... 9 45 a.m.	21 14 35	6th ... 9 0 a.m.	1	69 47 49	3·5971	9·7751	10·4160	,,
18th ...10 34 a.m.	21 37 15	,, 3 30 p.m.	2	69 50 36	......	......	......	,,
12th ... 6 0 p.m.	21 34 35	10th ... 9 0 a.m.	1	69 47 19	3·5877	9·7455	10·3849	,,
18th ... 9 10 a.m.	21 37 30	....	...	....	......	......	......	,,
12th ... 9 33 a.m.	21 59 0	11th ... 6 0 p.m.	1	69 45 32	3·5950	9·7582	10·3990	,,
16th ... 9 20 a.m.	21 46 55	12th ... 9 0 a.m.	2	69 47 26	......	......	......	,,
9th ... 9 15 a.m.	21 57 30	10th ... 8 30 a.m.	1	69 46 47	3·5995	9·7715	10·4134	,,
....	....	,, 9 0 a.m.	2	69 46 32	......	......	......	,,
18th ... 4 15 p.m.	22 15 0	11th ... 9 0 a.m.	1	69 48 47	3·5999	9·7951	10·4356	,,
,, 6 0 p.m.	21 57 30	,, 9 30 a.m.	2	69 49 42	......	......	......	,,
....	....	7th ... 3 30 p.m.	1	69 44 10	3·6055	9·7659	10·4102	,,
....	....	....	...	....	......	......	......	,,
6th ... 7 0 p.m.	22 1 40	5th ... 3 0 p.m.	1	69 44 40	3·5975	9·7420	10·3849	,,
....	....	,, 3 30 p.m.	2	69 43 10	......	......	......	,,
19th ... 5 39 p.m.	21 59 35	12th ... 9 30 a.m.	1	69 47 21	3·5927	9·7560	10·3963	,,
20th ... 5 35 p.m.	22 6 10	27th ... 9 0 a.m.	1	69 46 40	......	......	......	,,
10th ... 9 50 a.m.	22 23 40	6th ... 9 30 a.m.	1	69 46 47	3·5942	9·7402	10·3829	,,
15th ... 9 0 a.m.	22 28 40	,, 10 0 a.m.	2	69 42 44	......	......	......	,,
10th ... 9 0 a.m.	22 38 55	9th ... 8 30 a.m.	1	69 46 0	3·6004	9·7665	10·4090	,,
,, 9 2 a.m.	22 36 40	,, 9 30 a.m.	2	69 45 36	......	......	......	,,
16th ... 9 0 a.m.	22 38 0	15th ... 9 0 a.m.	1	69 46 23	3·5963	9·7600	10·4014	,,
....	....	....	...	....	......	......	......	,,
		Means for 1864.........		69 46 34	3·5967	9·7616	10·4031	

Note by the President.

Mr. Sidgreaves's observations, combined with those made at the same spot in 1858 in the course of the second magnetic survey of England, supply the materials for a first approximate deduction of the present amount of the secular change of magnetic dip and of the total magnetic Force at Stonyhurst.

Commencing with the Dip :—the results in 1858 were as follows (Brit. Assoc. Reports, 1861, pp. 253 & 254) :—

Sept. 20.	Kew Circle No. 30.	Needle 1	70°	0′	12″	Rev. W. Kay.
Nov. 2.	Kew Circle No. 32.	Needle 1	69	57	44	Rev. A. Weld.
„ 14.	„	„	70	3	30	„
„ 14.	„	„	70	4	21	„
Mean : corresponding in date to 1858.8			70	1	27	
And by the present Observations, corresponding in date to 1864.5			69	46	34	
Difference, corresponding to 5.7 years				14	53	

whence we have an annual secular decrease of 2′·614 ; mean epoch 1861.9.

In a memoir presented to the Royal Society in 1861, "On the Secular Change of the Dip in London between 1821 and 1860," printed in vol. xi. of the 'Proceedings,' pp. 144–162, the mean annual secular decrease of the dip in the years from 1821.65 to 1859.5 is stated to have been 2′·69, mean epoch 1840.6 ; and in the 21.2 years between 1838.3 and 1859.5, 2′·63 ; mean epoch 1848.9.

Proceeding to the Total Force :—its value obtained by myself at Stonyhurst by experiments of deflection and vibration with the Survey Collimator No. 5, in October 1858 was 10·385 in British units (Brit. Assoc. Reports, 1861, pp. 264, 268) ; and by the experiments of Mr. Sidgreaves with the apparatus belonging to Stonyhurst College (originally obtained from Kew), its mean value in 1864, derived from the twelve monthly determinations, was 10·4031 ; the difference is ·0181 in 5.75 years, or an annual increase of 0031. To compare with this, we have the statement in the British Survey (Brit. Assoc. Reports, 1861, p. 273), that from the absolute measures made monthly at Kew between April 1857 and March 1862 the total force had increased at Kew during that interval at an average annual rate of ·0025. In the same memoir it was also inferred, from a general comparison of the isodynamic lines in the first and second British Surveys, that along a line drawn in a N.W. and S.E. direction the secular change would be found contemporaneously somewhat greater at a northern or north-western station than at a southern or south-eastern station—greater therefore at Stonyhurst than at Kew. The general fact that the value of the total force in Britain is progressively increasing, may be inferred alike by the observations at Kew and at Stonyhurst ; the precise amount of the annual increase at either station will require a longer continuance of the same careful and systematic observations as those at Kew and Stonyhurst.

II. "Chemical Examination of the Fluid from the Peritoneal Cavity of the Nematode Entozoa." By Dr. W. Marcet, F.R.S. Received January 24, 1865.

Some time ago Dr. Cobbold sent me a quantity of fluid which he had extracted from about seventy perfectly fresh specimens of the *Ascaris megalocephala* of the horse, and he requested me to make an analysis of it. I most willingly availed myself of this unusual opportunity of ascertaining the composition of this fluid, the sample procured by Dr. Cobbold being fortunately large enough for the purpose.

The analysis of this fluid is interesting as showing that its composition is similar to that of juice of flesh in the higher animals, and consequently that the process of assimilation occurs in these worms much in the same way as in those animals where the organs of digestion and circulation are perfectly developed. It also shows that a fluid similar to that existing in muscular tissue is apparently elaborated by the intestines of the *Ascarides*, while in the higher animals this fluid is formed from the blood.

The fluid was turbid, of a pale yellow colour, and emitted an offensive odour, although not of decomposition.

Microscopical Examination.

Principally fine granular matter; a few elongated bodies, some convoluted, as if consisting of this granular matter cast by passage through a membranous tube. Some, but very few, spiral vegetable fibres and scales.

Chemical Examination.

Specific gravity 1·029, reaction slightly acid. 5 cubic centimetres were evaporated to dryness, which yielded, in 1000 parts,

Solid residue	82·7
Water	917·3
	1000·0

The fluid, when nearly boiling, coagulated into a solid mass, it therefore contained a large quantity of albumen.

With the object of separating the colloid from the crystalloid constituents, I measured off 10 cub. centims. of the liquid and dialyzed it for twenty-four hours in a 6-inch dialyzer. By this operation the fluid lost its acid reaction, becoming neutral; it has also parted with its smell.

The Colloid Fluid.—The solution remaining on the dialyzer consisted principally of albumen; it was evaporated to dryness, and the weight of the residue determined; this amounted to 0·532 grm., being 53 per 1000 of the fluid analyzed. The total solid constituents of the fluid being 82·7 per 1000, it will be seen at once that about $\frac{2}{3}$rds of the total residue consisted of colloid substances, and $\frac{1}{3}$rd of crystalloid. These numbers should be accepted as approximate results, there being no substance possessed of

absolutely colloid or crystalloid properties, and a small proportion of colloid having probably found its way through the membrane.

The dry colloid residue was incinerated, and found to contain 1·9 per cent. of ashes, which is so small a proportion as to show that very nearly the whole of the inorganic constituents of the fluid had passed through the membrane of the dialyzer.

The Crystalloid Fluid.—This consisted of the solution in distilled water of those constituents of the ascaris-fluid which had found their way through the diaphragm of the dialyzer. It contained no albumen but some organic matters, and very nearly the whole of the inorganic salts of the original fluid. Evaporated nearly to dryness, a mass of crystals appeared after a lapse of time in the thick residue. A part of the residue being ignited left a large proportion of ash, which was found to consist nearly entirely of phosphoric acid and potash. The aqueous solution of the ash reacted strongly alkaline, and emitted no carbonic acid when tested with a mineral acid, showing that there existed more phosphoric acid than was necessary to combine with the whole of the bases present. The absence of sulphates, of more than traces of chlorides, and of lime was very remarkable; there might have been some soda present, but potash greatly predominated. There is no record in my note-book as to the presence or absence of magnesia.

I now submitted to examination a solution in distilled water of the crystalloid residue. It reacted acid; the addition of a solution of nitrate of silver gave an abundant white precipitate with a slight yellow tinge, the fluid being acid before and after precipitation. There was therefore but a small proportion of common tribasic phosphate of potash present, and there appeared to be a much larger proportion of the bibasic phosphate; the former giving a yellow, and the latter a white precipitate with nitrate of silver.

I finally determined the fatty matters present in a given weight of the original fluid, and found that 1000 parts of the ascaris-fluid contained 5·1 parts of fat.

Conclusions.

We may conclude from this inquiry that nutrition in the nematode worms can be carried on by means of a fluid containing few other substances besides albumen and phosphate of potash. If we now consider that the principal constituent of juice of flesh is phosphate of potash, both tribasic and bibasic, that the ascaris- and flesh-fluid are both acid, that in both fluids there is a very small quantity of chlorides with but very little or no soda and little or no lime, we shall be able to draw a very interesting parallel between the assimilation in the highest and lowest animals.

III. "On the Commissures of the Cerebral Hemispheres of the Marsupialia and Monotremata, as compared with those of the Placental Mammals." By W. H. Flower, F.R.S. Received January 24, 1865.

(Abstract.)

As it is most convenient to pass from the best to the least known, and especially as the terms used in describing the anatomy of the vertebrated animals have in most cases been originally bestowed upon parts of the human body, the Paper commences by a short description of the septum ventriculorum and commissures of the human brain. This is done with a view to establish clearly, both by their structure and development, the mutual relations of the great transverse commissure or corpus callosum and the fornix. The latter is defined as essentially a longitudinal commissure, consisting of two lateral halves closely applied for a short space in the middle line, but each half belonging to its own hemisphere, and formed out of the longitudinal fibres bordering the superior margin of the ventricular aperture. There are no transverse fibres in the fornix proper, the so-called "psalterial fibres" connecting together the two hippocampi majores being a portion of the same system of fibres as the corpus callosum.

The relations of these parts are shown in a series of longitudinal and vertical sections of the brains of the Sheep, Rabbit, Two-toed Sloth, and Hedgehog among Placental Mammals, and in the same way in the Kangaroo, Wombat, and Thylacine among Marsupials, and the Echidna among Monotremes.

After reference to the literature of the subject, more especially to the writings of Professor Owen, whose statement (Phil. Trans. 1837) of the absence in the marsupials of the "corpus callosum," or "great transverse commissure which unites the supra-ventricular masses of the hemispheres," in all placentally developed mammals * has been almost universally adopted, the author proceeds to sum up the result of the present investigation as follows.

At the outset a confirmation is afforded of the important fact, first observed by Professor Owen, that the brains of animals of the orders Marsupialia and Monotremata present certain special and peculiar characters, by which they may be at once distinguished from those of other mammals. The appearance of either a transverse or longitudinal section would leave no doubt whatever as to which group the brain belonged. In

* In the paper by the same author "On the Characters, Principles of Division, and Primary Groups of the Class Mammalia" (Proc. Linn. Soc. 1858), the Subclass *Lyencephala* ("loose" or "disconnected" brain), equivalent to the Marsupialia and Monotremata, are characterized as having "the cerebral hemispheres but feebly and partially connected together by the 'fornix' and 'anterior commissure,' while in the rest of the class a part called 'corpus callosum' is added, which completes the connecting or commissural apparatus."

the differentiating characters to be enumerated, some members of the higher section present an approximation to the lower; but, as far as is known at present, there is still a wide interval between them without any connecting link.

The differences are manifold, but all have a certain relation to, and even a partial dependence on, each other. They may be enumerated under the following heads:—

1. The peculiar arrangement of the folding of the inner wall of the cerebral hemisphere. A deep fissure, with corresponding projection within, is continued forwards from the hippocampal fissure, almost the whole length of the inner wall.

2. The altered relation (consequent upon this disposition of the inner wall) and the very small development of the upper transverse commissural fibres (corpus callosum).

3. The immense increase in amount, and probably in function, of the inferior set of transverse commissural fibres (anterior commissure).

Each of these propositions must now be considered a little more closely. Arguing from our knowledge of the development of the brain in placental mammals (for of that of the marsupials we have at present no information), it may be supposed that the first-named is also first in order of time in the gradual evolution of the cerebral structures. Before any trace of the budding out of the fibres which shoot across the chasm separating the hollow sac-like hemispheres, before the differentiation of a portion of the septal area into the anterior commissure, that remarkable folding of the inner wall, indicated by the deep (hippocampal) furrow on the surface and the corresponding rounded projection in the interior, has already become distinctly manifest.

Now the first rudiment of the upper transverse commissure is found undoubtedly at the spot, afterwards situated near its middle, to which in the lowest placental mammals it is almost entirely confined. This spot is situated a little way above and in front of the anterior end of the ventricular aperture, at the upper edge of the region of adherence of the two hemispheres (the future septal area). In the placental mammals this part is in direct relation to the great mass of the internal medullary substance of the hemispheres, which has to be brought into communication. In the marsupials, on the other hand, the prolonged internal convolution or hippocampus spreading up to and beyond this point, forms the inner wall of the hemisphere from which the fibres pass across, and it is necessarily through the medium of this convolution, and following the circuitous course of its relief in the ventricle, that the upper part of the hemisphere can alone be brought into connexion with its fellow.

Can this transverse commissure, of which the relation is so disturbed by the disposition of the inner wall of the hemisphere, be regarded as homologous to the entire "corpus callosum" of the placental mammals? or is it, as has been suggested, to be looked upon as only representing the

psalterial fibres or transverse commissure of the hippocampi? Undoubtedly a large proportion of its fibres do come under the latter category. But even if they should nominally be all so included, it is important to bear in mind that we have still a disposition in the marsupial brain very different from that which would remain in the brain of any placental mammal after the upper and main part of the corpus callosum had been cut away. In the latter case the commissure of a very small part of the inner wall of the hemisphere alone is left, that part folded into the hippocampus. In the former there is a commissure, feeble it may be, but *radiating over the whole of the inner wall, from its most anterior to its posterior limits.* Granted that only the psalterial fibres are represented in the upper commissure of the marsupial brain, why should the name of "corpus callosum" be refused to it? These fibres are part of the great system of transverse fibres bringing the two hemispheres into connexion with each other; they are inseparably mingled at the points of contact with the fibres of the main body of the corpus callosum, and are only distinguished from it in consequence of the peculiar form of the special portions of the hemisphere they unite. Indeed they are scarcely more distinct than is the part called "rostrum" in front. And although, like the fibres of the hinder end of the corpus callosum, they blend at each extremity with the fibres of the diverging posterior crura of the fornix, they certainly cannot be confounded with that body, which, as shown before, is essentially a longitudinal commissure.

But is not the main part of the "corpus callosum" of the placental mammals also represented by the upper and anterior part of the transverse band which passes between the hemispheres of the marsupial brain and radiates out in a delicate lamina above the anterior part of the lateral ventricle? The most important and indeed crucial test in determining this question, is its position in regard to the septum ventriculorum, and especially the precommissural fibres of the fornix. Without any doubt in all marsupial and monotreme animals examined (sufficient to enable us to affirm without much hesitation that the character is universal) it lies *above* them, as distinctly seen in the transverse sections. This is precisely the same relationship which obtains in Man and all other mammalia, and this is one of the chief points in which not only the interpretation of facts but the observation of them recorded in the present paper differs from that of Professor Owen.

The defective proportions of the part representing the great transverse commissure of the placental mammals, which appear to result from or to be related to the peculiar conformation of the wall of the hemisphere, must not lead to the inference that the great medullary masses of the two halves of the cerebrum are by any means "disconnected." The want of the upper fibres is compensated for in a remarkable manner by the immense size of the anterior commissure, the fibres of which are seen radiating into all parts of the interior of the hemisphere. There can be little doubt that the development of this commissure is, in a certain measure, comple-

mentary to that of the corpus callosum. This is, moreover, a special characteristic of the lowest group of the mammalia, most remarkable because it is entirely lost in the next step of descent in the vertebrated classes.

After a description of the brain of a bird, the conclusion is arrived at that, great as is the difference between the placental and implacental mammal in the nature and extent of the connexion between the two lateral hemispheres of the cerebrum, it is not to be compared with that which obtains between the latter and the oviparous vertebrate.

IV. "Note on the Atomicity of Aluminium." By Professor A.W. Williamson, F.R.S., President of the Chemical Society. Received February 6, 1865.

In the "Preliminary Note on some Aluminium Compounds," by Messrs. Buckton and Odling, published in the last Number of the Society's 'Proceedings,' some questions of considerable theoretical importance are raised in connexion with the anomalous vapour-densities of aluminium ethyle and aluminium methyle. The authors have discovered that the vapour of aluminium methide ($Al^2 Me^6$) occupies rather more than two volumes ($H=1$ vol.) at 163°, when examined by Gay-Lussac's process, under less than atmospheric pressure. The boiling-point of the compound under atmospheric pressure is given at 130°, and the compound accordingly boiled a good deal below 130° at the reduced pressure at which the determination was made. The vapour was therefore considerably superheated when found to occupy a little more than two volumes. When still further superheated up to 220° to 240°, it was found to possess a density equivalent to rather less than four volumes at the normal temperature and pressure.

The aluminium ethyle was found to have a density decidedly in excess of the formula $Al^2 Et^6 = 4$ vols., but far too small for $Al^3 Me^6 = 2$ vols. From their analogy to aluminic chloride, $Al^2 Cl^6 = 2$ vols., the methide and ethide might be expected to have vapour-volumes corresponding to $Al^2 Me^6 = 2$ vols., $Al^2 Et^6 = 2$ vols. The authors seem, however, more inclined to doubt the truth of the general principles which lead us to consider these hexatomic formulæ the correct ones, than to doubt their own interpretation of the observations already made upon the new compounds.

Even if the vapour-volume of aluminic chloride had been unknown to us, there were ample grounds for assigning to aluminium methide a molecular formula $Al^2 Me^6$, and a vapour-density corresponding to $Al^2 Me^6 = 2$ vols.; for the close analogy of aluminic and ferric salts is perfectly notorious, and the constitution $Fe^2 O^3$ for ferric oxide settles $Al^2 O^3$ as the formula for alumina. With regard, however, to the chlorides of these metals, it might be supposed that the formula $Fe Cl^3$ and $Al Cl^3$ would be the most probable molecular formulæ; and Dr. Odling, in his useful Tables of Formulæ, published in 1864, expressed an opinion in favour of these formulæ by classing

as anomalous Deville's vapour-densities, which correspond to the higher formulæ $Al^2 Cl^6$, $Fe^2 Cl^6$. It is well known that Laurent and Gerhardt, whose penetrating minds raised so many vital questions of chemical philosophy, laid down a preliminary rule that every molecule must contain an even sum of the atoms of chlorine, hydrogen, nitrogen, and metals. According to this rule, the formulæ $Al^2 Cl^6$ and $Fe^2 Cl^6$ would have no greater probability than the formulæ $Fe Cl^3$, $Al Cl^3$; and judging by that rule, Dr. Odling naturally preferred the simpler formulæ.

Since Gerhardt's time chemists have, however, extended to the greater number of metals the arguments which proved oxygen to be biatomic; and we now know that the alkali-metals, the nitrogen series, silver, gold, and boron, may count with the atoms of chlorine, hydrogen, &c. to make up an even number in each molecule, but that the greater number of metals must not be so counted; for that in each molecule in which they are contained the sum of the atoms of chlorine, hydrogen, nitrogen, potassium, &c. must be even, just as much as if the atom of the diatomic or tetratomic metal were not in the compound. In a paper "On the Classification of the Elements in relation to their Atomicities," I had occasion to point out that inasmuch as iron and aluminium belong, partly by their own properties, partly by their analogies, to the class of metals which do not join with chlorine, &c. in making up an even number of atoms, the number of those other atoms in each molecule must be even in itself, just as if iron or aluminium were not there; and that accordingly the formulæ $Fe^2 Cl^6$, $Al^2 Cl^6$ are really quite normal. In like manner I showed that the vapour-density of calomel, $HgCl = 2$ vols., is anomalous, as containing in a molecular volume a single atom of chlorine, although, in accordance with Gerhardt's rule, Dr. Odling had classed it as normal. I certainly understood that my able friend accepted my suggestion in this case at least, for he speedily brought forward theoretical and experimental facts in confirmation of it.

These examples serve to show that it was to be expected that the ethyle and methyle compounds of aluminium would contain an even number of atoms of ethyle and methyle in each molecule, and that their formulæ would accordingly be $Al^2 Me^6$, $Al^2 Et^6$.

It remains for us to consider how the deviation from our theoretical anticipations in the case of aluminium ethyle and the partial deviation in the case of aluminium methyle ought to be treated.

Fortunately we have the benefit of some experience to guide us in this matter, for a considerable number of other compounds have been found to occupy in the state of vapour nearly double the volume which corresponds to one molecule; but, with very few exceptions, all of them have already been proved to have undergone decomposition, so as to consist of two uncombined molecules. Thus sal-ammoniac is admitted to have the molecular formula $NH^4 Cl$; yet in the state of vapour this quantity occupies the volume of nearly two molecules, viz. four volumes. Has the anomaly led us to doubt the atomic weight of chlorine, nitrogen, or hy-

drogen, or to doubt any other of the results of our comparison of their compounds? or has it led chemists to diffusion experiments with its vapour, proving it to contain uncombined HCl and NH^3, each occupying its own natural volume? Has it not been proved that at the temperature at which sal-ammoniac vapour was measured, its constituents mix either without evolving heat (that invariable function of chemical action), or, according to another experimentalist, with evolution of far less heat than of the whole quantity of hydrochloric acid and ammonia combined, on coming together at that high temperature?

Again, $SO^4 H^2$ is known to represent the formula of one molecule of hydric sulphate, yet the vapour formed from it occupies nearly the bulk of two molecules. Has this fact cast any doubt on the atomic weights of the elements S, O, or H? Or has it led to the discovery of peculiarities in the constitution of the vapour which would probably have escaped notice had they not been anticipated by theory, peculiarities which go a long way towards bringing the apparent anomalies within the law?

Nitric peroxide, $N^2 O^4$, was considered, from our knowledge of other volatile compounds of nitrogen, to be anomalous in its vapour-volume being $N^2 O^4 = 4$ vols.; and we have been shown by the experiment of Messrs. Playfair and Wanklyn, that the anomaly almost disappears when the compound is evaporated by the aid of a permanent gas at a temperature considerably below its boiling-point, as its theoretical molecule $N^2 O^4$ is then found to occupy the two volumes which every undecomposed molecule occupies. This explanation seems to me to be the more entitled to grave consideration on the part of the discoverers of the new aluminium compounds, from the fact that the evidence in favour of it has been admitted to be conclusive by Dr. Odling, who classes nitric peroxide by the formula $N^2 O^4 = 2$ vols. among compounds with normal vapour-densities, in virtue of the fact that at low temperatures it can be obtained with that density, though having half that density at higher temperatures.

The arguments for admitting that the low vapour-densities of the aluminium compounds are anomalous are even stronger than those which are admitted in the case of nitric peroxide; for it did require very severe superheating to get the aluminium compounds to near four volumes, whereas it required very ingenious devices to get nitric peroxide out of the four-volume state.

Such guiding principles as we have acquired in chemistry are the noblest fruits of the accumulated labours of numberless patient experimentalists and thinkers; and when any new or old fact appears to be at variance with those principles, we either add to our knowledge by discovering new facts which remove the apparent inconsistency, or we put the case by for a while and frankly say that we do not understand it.

The decision of the atomic weight of aluminium has involved greater difficulty than was encountered in the case of most other metals, owing to the fact of our knowing only one oxide of the metal, and salts correspond-

ing to it; but the analogies which connect aluminium with other metals are so close and so numerous, that there are probably few metals of which the position in our classification is more satisfactorily settled. We may safely trust that the able investigators who are examining these interesting compounds will bring them more fully than now within the laws which regulate the combining proportions of their constituent elements; for, as it now stands, the anomaly is far less than many others which have been satisfactorily explained by further investigations.

Meanwhile aluminium is a metal singular for only appearing in that pseudo-triatomic character in which iron and chromium appear in their sesquisalts.

February 16, 1865.

Dr. W. A. MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "On the Synthesis of Tribasic Acids." By MAXWELL SIMPSON, M.D., F.R.S. Received January 25, 1865.

(Abstract.)

In a former Number of the 'Proceedings'* I gave a preliminary notice of a tribasic acid having the composition $C_{12} H_8 O_{12}$, formed by the action of potash on tercyanide of allyle. The process for the preparation of the acid given in that paper I have since succeeded in improving very considerably, so that I can now obtain it in quantity and with tolerable facility. An account of the improved process is contained in the general paper which accompanies this abstract. The paper also contains a description of the crystalline form of the acid, for which I am indebted to Professor Miller of Cambridge.

M. Kekulé† proposes to call this body carballylic acid. This name I cannot, however, accept without some modification, as recent researches‡ have proved that it belongs by right to crotonic acid. I propose therefore, in order to avoid confusion, to call it *tri*carballylic acid.

Since the appearance of my preliminary paper, I have also prepared and analyzed several of the salts and ethers of this acid, of which the following is a short account.

Tricarballylic Ether, $\left.\begin{matrix} C_{12} H_5 O_6''' \\ (C_4 H_5)_3 \end{matrix}\right\} O_6$.

This ether is readily prepared by conducting a stream of dry hydrochloric acid gas into a solution of tricarballylic acid in absolute alcohol. The product obtained on evaporating the alcohol distils between 295° and 305° C.

* Proceedings of the Royal Society, vol. xii. p. 236.

† Lehrbuch der Organischen Chemie, vol. ii. p. 187.

‡ Annalen der Chemie und Pharmacie, vol. cxxxi. p. 58.

It is a colourless liquid, is slightly soluble in water, and has an acrid taste. Heated with solid potash it suffers decomposition, alcohol being formed and the acid regenerated.

Tricarballylic Amylic Ether, $\left.\begin{matrix} C_{12}H_5O_6''' \\ (C_{10}H_{11})_3 \end{matrix}\right\} O_6.$

This body is formed when dry hydrochloric acid gas is passed into a mixture of one part by weight of tricarballylic acid and two parts of pure amylic alcohol maintained at the temperature of boiling water. The product may be partially purified by heating it in a retort till 200° C., and then dissolving it successively in alcohol and in ether. It is a thick oily liquid, is heavier than water, and has an acrid taste. Its boiling-point is beyond the range of the mercurial thermometer. Heated with solid potash, it is resolved into amylic alcohol and tricarballylic acid.

Glyceri-tricarballylate of Baryta, $\left.\begin{matrix} C_{12}H_5O_6''' \\ C_6H_7O_4' \\ Ba_2 \end{matrix}\right\} O_6.$

This salt was prepared by maintaining for several hours at the temperature of 200° C. in a sealed tube a mixture of one part of tricarballylic acid and two parts of pure glycerine. The product was neutralized by a solution of baryta, evaporated to dryness, and digested with absolute alcohol to remove the uncombined glycerine. A buff-coloured powder was thus obtained having, I have no doubt, the composition expressed by the above formula, although my analyses do not correspond very well with it. The acid combined with the baryta is bibasic, and is represented by the formula

$$\left.\begin{matrix} C_{12}H_5O_6' \\ C_6H_7O_4' \\ H_2 \end{matrix}\right\} O_6.$$

Soda-salts of Tricarballylic Acid.

The soda-salts of this acid are very soluble in water and difficult to crystallize. Three salts may, I believe, be found, containing respectively one, two, and three equivalents of sodium. One equivalent of the acid I found required for complete neutralization exactly three equivalents of pure carbonate of soda. The composition of the salt with two equivalents of sodium which I obtained in crystals is probably $\left.\begin{matrix} C_{12}H_5O_6''' \\ Na_2 \\ H \end{matrix}\right\} O_6+4HO.$

Tricarballylate of Lime, $\left.\begin{matrix} C_{12}H_5O_6''' \\ Ca_3 \end{matrix}\right\} O_6+4HO.$

When a solution of this acid is neutralized with lime-water and evaporated, a white amorphous powder separates, which is the salt in question. It is sparingly soluble in water, and freely soluble in dilute acids.

Tricarballylate of Copper, $\left.\begin{matrix} C_{12}H_5O_6''' \\ Cu_3 \end{matrix}\right\} O_6.$

This salt falls in the form of a beautiful bluish-green powder when sul-

phate of copper is added to a hot solution of tricarballylate of soda. It is insoluble in water, but soluble in dilute acids.

Tricarballylate of Lead, $\left.\begin{matrix} C_{12}H_5O_6''' \\ Pb_3 \end{matrix}\right\} O_6.$

This salt precipitates when an excess of acetate of lead is added to a solution of tricarballylate of soda. It is a white powder insoluble in water, but soluble in dilute nitric acid.

The composition of the foregoing salts and ethers fully confirms the view I took of the basicity of this acid in my preliminary paper. It is, I believe, at present the sole representative of its class. It will not, however, I believe, long remain so, as the process by which it has been obtained will, I have no doubt, be found to be of general application.

This acid bears the same relation to citric acid that succinic bears to malic acid:—

$C_{12}H_8O_{12}$, tricarballylic acid.	$C_8H_6O_8$, succinic acid.
$C_{12}H_8O_{14}$, citric acid.	$C_8H_6O_{10}$, malic acid.

That this relationship exists not only on paper, but also in the nature of the bodies themselves, is, I think, highly probable. In order to arrive at certainty on this point, I have endeavoured, by the addition of two equivalents of oxygen, to transform tricarballylic into citric acid. My researches in this direction have not hitherto been attended with success.

II. "Notes of Researches on the Acids of the Lactic Series.—No. III. Action of Zincethyl upon Ethylic Leucate." By E. Frankland, F.R.S., and B. F. Duppa, Esq. Received February 1, 1865.

In describing the production of ethylic leucate or diethoxalate *, formed when zincethyl acts upon ethylic oxalate, we assumed the intermediate formation of zincoleucic ether, and explained the reaction by the following equation, in which zinc is regarded as a monatomic metal:—

$$C_2\left\{\begin{matrix} O \\ O\,C_2H_5 \\ O \\ O\,C_2'H_5 \end{matrix}\right. + Zn'_2\left\{\begin{matrix} C_2H_5 \\ C_2H_5 \end{matrix}\right. = C_2\left\{\begin{matrix} (C_2H_5)_2 \\ OZn' \\ O \\ O\,C_2H_5 \end{matrix}\right. + \left.\begin{matrix} Zn' \\ C_2H_5 \end{matrix}\right\} O.$$

Ethylic oxalate. Zincethyl. Zincoleucic ether. Zincethylate.

In contact with water we conceived zincoleucic ether to be decomposed with the formation of ethylic leucate and zinc hydrate,

$$C_2\left\{\begin{matrix} (C_2H_5)_2 \\ OZn' \\ O \\ O\,C_2H_5 \end{matrix}\right. + H_2O = C_2\left\{\begin{matrix} (C_2H_5)_2 \\ OH \\ O \\ O\,C_2H_5 \end{matrix}\right. + \left.\begin{matrix} Zn' \\ H \end{matrix}\right\} O.$$

Zincoleucic ether. Ethylic leucate.

Since these reactions were thus expressed, zinc has come to be generally

* Proceedings of the Royal Society, vol. xii. p. 396.

regarded as a diatomic metal, a circumstance which has led us to study the action of zincethyl upon ethylic leucate, with a view to the more satisfactory elucidation of the above changes, which finally result in ethylic leucate. Assuming the diatomicity of zinc, which can now no longer be doubted, it is obvious that by the loss of one atom of ethyl, zincethyl will, like many other organo-metallic bodies, pass from a condition of perfect to one of partial saturation, in which it will play the part of a monatomic radical.

That zincethyl, in being acted upon by oxygen, passes through two distinct stages of change has been already indicated by one of us in describing the reactions of that body*; for when a current of dry oxygen is made to pass through an ethereal solution of zincethyl, dense white fumes continue to fill the atmosphere of the vessel, until about one-half of the total quantity of oxygen necessary for the complete oxidation of the zincethyl has been taken up. Then the white fumes entirely cease, showing the absence of free zincethyl, and at the same moment the liquid, which up to that time had remained perfectly transparent, begins to deposit a copious white precipitate, which continues to increase until the remaining half of the oxygen is absorbed. If the process of oxidation be arrested when the white fumes cease to be formed, the product effervesces violently when mixed with water, owing to the escape of hydride of ethyl; but when the oxidation is completed, the white solid mass produced consists chiefly of zincethylate, and does not in the slightest degree effervesce in contact with water. The two stages of this reaction depend essentially upon the successive linking of the zinc with the two atoms of ethyl by means of diatomic oxygen. The first stage of oxidation is expressed by the following equation:—

$$\underset{\text{Zincethyl.}}{Zn''\left\{\begin{matrix} C_2H_5 \\ C_2H_5 \end{matrix}\right.} + O = \underset{\text{Zincethylo-ethylate.}}{Zn''\left\{\begin{matrix} C_2H_5 \\ OC_2H_5 \end{matrix}\right.}.$$

The zincethylo-ethylate thus formed is perfectly soluble in ether, and is instantly decomposed by water according to the following equation:—

$$\underset{\text{Zincethylo-ethylate.}}{Zn''\left\{\begin{matrix} C_2H_5 \\ OC_2H_5 \end{matrix}\right.} + 2H_2O = \underset{\text{Zinc hydrate.}}{Zn''\left\{\begin{matrix} OH \\ OH \end{matrix}\right.} + \underset{\text{Alcohol.}}{\left.\begin{matrix} C_2H_5 \\ H \end{matrix}\right\}O} + \underset{\text{Ethylic hydride.}}{\left.\begin{matrix} C_2H_5 \\ H \end{matrix}\right\}}.$$

Treated with dry oxygen, zincethylo-ethylate, in ethereal solution, absorbs a second atom of that element, and it is this further absorption that constitutes the second stage above referred to, resulting in the production of ethylate of zinc,

$$Zn''\left\{\begin{matrix} C_2H_5 \\ OC_2H_5 \end{matrix}\right. + O = Zn''\left\{\begin{matrix} OC_2H_5 \\ OC_2H_5 \end{matrix}\right..$$

Wanklyn† was the first clearly to point out the probable existence of zincmonethyl, or rather its homologue zinc-monomethyl, indicating at the

* Phil. Trans. 1855, p. 268.

† Journal of Chem. Soc. 1861, p. 127.

same time its radical functions, when he ascribed to the crystalline compound obtained in the preparation of zincmethyl the formula

$$Zn_2{}' \left\{ \begin{matrix} CH_3 \\ I \end{matrix} \right. .$$

In the same memoir he also represented this compound as the analogue of mercuric methiodide,

$$Hg'' \left\{ \begin{matrix} CH_3 \\ I \end{matrix} \right. .$$

More recently Buttlerow† has prominently drawn attention to this behaviour of organo-zinc compounds, and has succeeded in obtaining zincmethylo-methylate,

$$Zn'' \left\{ \begin{matrix} CH_3 \\ O\,CH_3 \end{matrix} \right. ,$$

in a condition approaching to purity, by passing a stream of dry air through a solution of zincmethyl in methyl iodide. M. Buttlerow's success in obtaining this body and his failure in converting it into zincmethylate, are both probably due to the comparative insolubility of zincmethylo-methylate in methyl iodide, owing to which the first product of oxidation was, to a great extent, protected from the further action of oxygen. When, however, ether is used as the solvent of zincethyl, the oxidized product remains in solution until the first stage is passed, after which zincethylate is gradually precipitated until the second stage is completed. Indeed, as has been shown in the memoir above referred to (Phil. Trans. 1855, p. 268), the oxidation, instead of stopping at the first stage, proceeds even somewhat further than the second, and the product formed does not possess a composition in any degree approaching that which M. Buttlerow asserts it to have. This is evident from the following numbers, and from the circumstance that it does not effervesce in the slightest degree when mixed with water:—

	Percentage composition according to Buttlerow's formula $\left. \begin{matrix} C_2H_5Zn \\ C_2H_5 \end{matrix} \right\}$	Percentage composition according to mean of analyses.
C	34·53	25·43
H	7·20	5·32
Zn	46·76	42·04
O	11·51	27·21
	100·00	100·00

The existence of monatomic organo-zinc radicals receives further support from the following experimental determinations, which also show the functions of such radicals in the formation of the ethers of the lactic series from ethylic oxalate.

When zincethyl is added to leucic ether previously cooled in a freezing mixture, each drop of the zinc compound, as it comes into contact with the ether, hisses like anhydrous phosphoric acid when dropped into water. Torrents of hydride of ethyl are evolved, and the mixture finally solidifies to a white tenacious mass, which fuses on the application of heat, and does

* Bulletin de la Soc. Chim. August 1864.

not distil below 100° C., at about which temperature a violent action sets in; a great quantity of gas is evolved, and the residue solidifies to a pitch-like mass, which, on treatment with water and subsequent distillation, yields about one-fourth of the leucic ether employed. If the above-mentioned white mass, instead of being heated, be mixed with water, it effervesces strongly, zinc hydrate is formed, and pure leucic ether separates in quantity nearly equal to that originally employed.

In a quantitative experiment 12·93 grms. of zincethyl were treated with leucic ether, excess being avoided; 15·67 grms. of leucic ether were required to saturate the above quantity of zincethyl, and the weight of hydride of ethyl evolved, which was carefully determined, amounted to 3·08 grms. These numbers closely agree with those deduced from the following equation:—

$$\underset{\text{Ethylic leucate.}}{C_2\left\{\begin{matrix}(C_2H_5)_2\\ \underline{OH}\\ O\\ OC_2H_5\end{matrix}\right.} + Zn''\left\{\begin{matrix}C_2H_5\\ C_2H_5\end{matrix}\right. = \underset{\text{Zincmonethyl-ethylic leucate.}}{C_2\left\{\begin{matrix}(C_2H_5)_2\\ \underline{OZnC_2H_5}\\ O\\ OC_2H_5\end{matrix}\right.} + \left.\begin{matrix}C_2H_5\\ H\end{matrix}\right\},$$

as seen from the following comparison:—

	Theoretical.	Experimental.
Leucic ether required to decompose 12·93 grms. of zincethyl	15·55 grms.	15·67 grms.
Weight of hydride of ethyl evolved ..	3·04 „	3·08 „

Zincmonethyl-ethylic leucate is a colourless viscous solid, soluble in ether, but apparently incapable of crystallization. It absorbs oxygen with avidity and in contact with water effervesces strongly, reproducing leucic ether according to the following equation:—

$$\underset{\text{Zincomethyl-ethylic leucate.}}{C_2\left\{\begin{matrix}(C_2H_5)_2\\ \underline{OZnC_2H_5}\\ O\\ O_2H_5\end{matrix}\right.} + 2H_2O = \underset{\text{Ethylic leucate.}}{C_2\left\{\begin{matrix}(C_2H_5)_2\\ \underline{OH}\\ O\\ OC_2H_5\end{matrix}\right.} + \left.\begin{matrix}C_2H_5\\ H\end{matrix}\right\} + Zn''\left\{\begin{matrix}OH\\ OH\end{matrix}\right.$$

Zincmonethyl-ethylic leucate combines energetically with iodine; an ethereal solution of the latter added to it is almost instantaneously decolorized and a large quantity of iodide of ethyl is produced. In continuation of the above quantitative experiment the following results were obtained. The product of the action of 12·93 grms. of zincethyl upon 15·67 grms. of leucic ether decolorized an ethereal solution containing 23·75 grms. of iodine, the quantity required by the following equation being 25·04 grms.,

$$\underset{\text{Zincmonethyl-ethylic leucate.}}{2C_2\left\{\begin{matrix}(C_2H_5)_2\\ \underline{OZnC_2H_5}\\ O\\ OC_2H_5\end{matrix}\right.} + I_4 = \underset{\text{Zincoleucic ether.}}{C_4\left\{\begin{matrix}C_2H_5)_4\\ O\\ \underline{O}^{Zn}\\ O_2\\ (OC_2H_5)_2\end{matrix}\right.} + ZnI_2 + 2C_2H_5I.$$

It was obviously impossible to collect in a state of purity the iodide of ethyl thus set at liberty without considerable loss; but the quantity of the pure iodide actually obtained was 12 grms. The above equation requires 14·6 grms.

On the removal of ether and iodide of ethyl, the mixture of zincoleucic ether and iodide of zinc forms a transparent gummy mass easily soluble in ether, bisulphide of carbon, and caoutchoucin, but totally incapable of crystallizing from any of its solutions. All our attempts to separate these bodies have hitherto proved abortive, and it is by no means improbable that they are chemically combined.

The action of zincethyl upon ethylic leucate throws much light upon the production of the latter from zincethyl and ethylic oxalate, and scarcely leaves a doubt that, when zincethyl is added to ethylic leucate, there is reproduced the zinc compound from which the ethylic leucate was first formed. We may therefore express the two stages in the original production of ethylic leucate by the following equations:—

$$\underset{\text{Ethylic oxalate.}}{C_2\left\{\begin{array}{l}O\\ OC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right.} + 2Zn''\left\{\begin{array}{l}C_2H_5\\ C_2H_5\end{array}\right. = \underset{\text{Zincmonethyl-ethylic leucate.}}{C_2\left\{\begin{array}{l}(C_2H_5)_2\\ OZnC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right.} + \underset{\text{Zincethylo-ethylate.}}{Zn''\left\{\begin{array}{l}C_2H_5\\ OC_2H_5\end{array}\right.}$$

$$\underset{\text{Zincmonethyl-ethylic leucate.}}{C_2\left\{\begin{array}{l}(C_2H_5)_2\\ OZnC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right.} + 2H_2O = \underset{\text{Ethylic leucate.}}{C_2\left\{\begin{array}{l}(C_2H_5)_2\\ OH\\ \hline O\\ OC_2H_5\end{array}\right.} + \underset{\text{Zinc hydrate.}}{Zn''\left\{\begin{array}{l}OH\\ OH\end{array}\right.} + \left.\begin{array}{l}C_2H_5\\ H\end{array}\right\}$$

III. "Notes of Researches on the Acids of the Lactic Series.—No. IV. Action of Zinc upon Oxalic Ether and the Iodides of Methyl and Ethyl mixed in atomic proportions." By E. Frankland, F.R.S. and B. F. Duppa, Esq. Received February 2, 1865.

The reaction of zinc upon oxalic ether in the presence of iodide of amyl, the results of which we hope very shortly to have the honour of laying before the Royal Society, had led us to anticipate, that if an oxalate of one organic radical were treated with the iodide of another, one atom of diatomic oxygen in the oxalic ether would be replaced by two different monatomic radicals. This anticipation was not, however, realized, as we have already shown*, when the reaction was extended to a mixture of iodide of ethyl with oxalate of methyl, and iodide of methyl with oxalate of ethyl. In both these cases the radicals presented in the form of iodides were the

* Proceedings of the Royal Society, vol. xiv. p. 17.

only ones entering into the composition of the resulting acids, which proved to have respectively the composition of leucic or diethoxalic acid,

$$C_2\left\{\begin{array}{l}(C_2H_5)_2\\ OH\\ \hline O\\ OH\end{array}\right.$$

and dimethoxalic acid,

$$C_2\left\{\begin{array}{l}(CH_3)_2\\ OH\\ \hline O\\ OH\end{array}\right.$$

Certain theoretical considerations, however, rendered it important for us to be able distinctly to label each radical entering into the composition of the derived acids, so as to enable us with certainty to trace its source, and, if possible, to determine its position or value in the resulting complex molecule. We therefore endeavoured to accomplish the desired end by acting with zinc upon a mixture consisting of one atom of oxalic ether and one atom each of the iodides of methyl and ethyl, by which we hoped to obtain an acid of the following composition:—

$$C_2\left\{\begin{array}{l}C_2H_5\\ CH_3\\ OH\\ \hline O\\ OH\end{array}\right.$$

Experiment completely proved the practicability of this reaction, and its result even exceeded our expectations, since not only was the ether corresponding to the above acid formed with the greatest facility, but it was produced almost to the complete exclusion of the ethers of leucic and dimethoxalic acids.

200 grms. of oxalic ether were mixed with the proper atomic proportions of iodide of methyl and iodide of ethyl, and were digested with granulated zinc for several days at a temperature of from 35° to 40° C., until the supernatant liquid became oily, and solidified to a crystalline mass on cooling. Water being now added until effervescence ceased, the whole was submitted to distillation in an oil-bath. With the exception of a small quantity of the mixed iodides of ethyl and methyl that had escaped decomposition, the distillate consisted of a homogeneous liquid composed of water, ethylic and methylic alcohols, and an ethereal body, which last was separated by repeated agitation with large volumes of ether and subsequent rectification. In this manner there was obtained a large quantity of a liquid which boiled constantly at 165°·5 C., and yielded on analysis numbers very closely corresponding with he formula

$$C_2\left\{\begin{array}{l}C_2H_5\\ CH_3\\ OH\\ \hline O\\ OC_2H_5\end{array}\right.$$

The production of this ether is explained in the following equations:—

$$C_2\left\{\begin{array}{l} O \\ OC_2H_5 \\ \hline O \\ OC_2H_5 \end{array}\right. + Zn''_4 + 2\left.\begin{array}{l} C_2H_5 \\ I \end{array}\right\} + 2\left.\begin{array}{l} CH_3 \\ I \end{array}\right\} = C_2\left\{\begin{array}{l} C_2H_5 \\ CH_3 \\ OZnCH_3 \\ \hline O \\ OC_2H_5H_5 \end{array}\right. + Zn''\left\{\begin{array}{l} C_2H \\ OC_2H_5 \end{array}\right. + 2ZnI_2$$

Oxalic ether. Zincmonethyl-ethomethoxalate of ethyl. Zincethylo-ethylate.

$$C_2\left\{\begin{array}{l} C_2H_5 \\ CH_3 \\ OZnCH_3 \\ \hline O \\ OC_2H_5 \end{array}\right. + 2H_2O = C_2\left\{\begin{array}{l} C_2H_5 \\ CH_3 \\ OH \\ \hline O \\ OC_2H_5 \end{array}\right. + \left.\begin{array}{l} C_2H_5 \\ H \end{array}\right\} + Zn''\left\{\begin{array}{l} OH \\ OH \end{array}\right.$$

Zincmonethyl-ethomethoxalate of ethyl. Ethylic ethomethoxalate. Zinc hydrate.

A not inconsiderable amount of the ether thus formed in this and in the analogous reactions described in our previous communications, appears to be decomposed by the zinc hydrate; at all events an appreciable quantity of the zinc-salt of the derived acid is always obtained from the residue left after distillation of the ethereal product.

Ethylic ethomethoxalate, as we propose to name the new ether, is a colourless, transparent, and mobile liquid, possessing a penetrating ethereal odour much resembling that of ethylic diethoxalate. It is very soluble in water, alcohol, and ether, and has a specific gravity of ·9768 at 13° C. It boils at 165°·5 C., and its vapour has a density of 4·98; the theoretical number for a two-volume vapour ($H_2O = 2$ vols.) of the above formula being 5·04.

Ethylic ethomethoxalate is readily decomposed, even by aqueous solutions of the alkalies and of baryta, yielding alcohol and a salt of the base. The ethomethoxalate of barium was thus prepared. It crystallizes from an aqueous solution as a beautiful radiated mass of silky lustre, very easily soluble in water. Its analysis gave numbers closely corresponding with the formula

$$C_4\left\{\begin{array}{l} (C_2H_5)_2 \\ (CH_3)_2 \\ (OH)_2 \\ \hline O_2 \\ \left.\begin{array}{l} O \\ O \end{array}\right. Ba'' \end{array}\right.$$

By exactly decomposing this salt with dilute sulphuric acid and evaporating the filtrate, first in a retort, and afterwards *in vacuo*, ethomethoxalic acid was obtained as a splendid white crystalline mass, fusing at 63° C., subliming readily at 100°, and condensing in magnificent star-like groups upon a cold surface. It boils with decomposition at 190° C. Ethomethoxalic acid is very readily soluble in ether, alcohol, and water; small fragments of it thrown upon water rotate like camphor whilst dissolving.

These solutions react powerfully acid, and readily decompose carbonates. The analysis of this acid gave numbers closely corresponding with the formula

$$C_2\left\{\begin{array}{l} C_2H_5 \\ CH_3 \\ OH \\ \hline O \\ OH \end{array}\right.$$

We have prepared silver ethomethoxalate by treating the free acid dissolved in water with carbonate of silver. This salt crystallizes in splendid mammillated masses half an inch in diameter, which are tolerably soluble in water. It gave numbers, on analysis, in accordance with the formula

$$C_2\left\{\begin{array}{l} C_2H_5 \\ CH_3 \\ OH \\ \hline O \\ OAg \end{array}\right.$$

February 23, 1865.

JOHN P. GASSIOT, Esq., Vice-President, in the Chair.

The following communications were read :—

I. "On New Cornish Minerals of the Brochantite Group." By Professor N. STORY MASKELYNE, M.A., Keeper of the Mineral Department, British Museum. Communicated by A. M. STORY MASKELYNE, M.A. Received February 13, 1865.

(Abstract.)

On a small fragment of Killas from Cornwall, I discovered, several months ago, a new mineral in the form of minute but well-formed crystals. The specimen had come from Mr. Talling, of Lostwithiel, a mineral-dealer, to whose activity and intelligence I am indebted for the materials that form the subject of this paper. After a little while he found the locality of the mineral, and sent me other and finer specimens; but these specimens proved to contain other new minerals besides the one already mentioned. Two of these minerals are described in this paper, and a third will form the subject of a further communication.

I. *Langite.*

The first of these minerals which I proceed to describe is one to which I have given the name of Langite, in honour of my friend Dr. Viktor von Lang, now of Gratz, and lately my colleague in the British Museum. It occurs in minute crystals, or as a crystalline crust on the Killas, of a fine blue with a greenish hue in certain lights. The crystals are prismatic. The forms observed are (1 0 0), (0 0 1), (1 1 0), and (2 0 1) & (0 1 0), the

normal inclinations giving the following angles, which are the averages of many measurements :—

$$1\,1\,0\ \bar{1}\,1\,0 = 56^\circ\ 16'$$
$$1\,0\,0\ 1\,1\,0 = 61\ 52$$
$$0\,0\,1\ 2\,0\,1 = 51\ 46$$

conducting to the parametral ratios

$$a : b : c = 1 : 0{\cdot}5347 : 0{\cdot}6346.$$

The crystals are twinned after the manner of cerussite, the twin axis being normal to the plane (1 1 0).

$$\bar{1}\,1\,0\ (1\,1\,0)\ \bar{1}\,1\,0 = 112^\circ\ 33'$$
$$1\,0\,0\ (1\,1\,0)\ 1\,0\,0 = 123\ 44$$
$$\bar{1}\,1\,0\ (1\,1\,0)\ 1\,\bar{1}\,0 = 67\ 26$$

Cleavages seem to exist parallel to 0 0 1 and 1 0 0. The planes 0 0 1 and 1 0 0 are very brilliant. The plane of the optic axes, as seen through a section parallel to the plane 0 0 1, is parallel to 1 0 0. The normal to 0 0 1 would seem to be the first mean line, and it is negative. The optical orientation of the mineral is therefore $\mathfrak{b}$, $\mathfrak{c}$, $\mathfrak{a}$.

The crystals are dichroic.

1. Seen along axis *c*, $\mathfrak{c}$, greenish blue.
   $\mathfrak{b}$, blue.
2. Seen along axis *a*, $\mathfrak{c}$, darker greenish blue.
   $\mathfrak{a}$, lighter bluish green.

The specific gravity of Langite is 3·48 to 3·50. Its hardness is under 3. It will not abrade calcite.

Before the blowpipe on charcoal it gives off water, and fumes and becomes reduced to metallic copper. Insoluble in water, it is readily dissolved by acids and ammonia. Heated, it passes through (1) a bright green, and (2) various tints of olive-green, till (3) it becomes black. Water is given off the whole time, and finally it has a strongly acid reaction.

The first stage corresponds to the loss of one equivalent of water; the second reduces its composition to that of Brochantite; at the third it loses all its water.

The chemical composition of Langite is represented by the formula $3Cu''\,H'_2\,O_2 + Cu''\,SO_4 + 2H'_2\,O$, which requires the following numbers :—

		Calculated percentage.	Average found.
4 equivalents of copper..........	126·72	=52·00	52·55
4 equivalents of oxygen	32·	=13·13	13·27
1 equivalent of sulphuric anhydride	40·	=16·41	16·42
5 equivalents of water	45·	=18·46	18·317
	243·72	100·00	100·56

I have met with a small and old specimen of Connellite with a twin crystal of Langite associated with it.

II. *Waringtonite.*

To a Cornish mineral associated with Langite, emerald to verdigris-green in colour, occurring in incrustations generally crystalline, and seen occasionally in distinct individual crystals aggregated loosely on the Killas, I have given the name of Waringtonite, in honour of my friend Mr. Warington Smyth. The crystals are always of the same form, that, namely, of a double-curved wedge. A narrow plane, 0 0 1, is very brilliant and without striation. It appears to be a cleavage-plane. A second, but scarcely measurable plane, 1 0 0, occurs at right angles to it, truncating the thin ends of the wedge. The prism planes in the zones 0 1 0, 0 0 1, and 0 1 0, 1 0 0 are uniformly curved. The planes of two prisms seem to exist in the zone 0 1 0, 0 0 1, but the angles, as approximately measured by the goniometer, are not very reliable; one of them, however, may be pretty confidently asserted to be very near 28° 30′, which is the mean of many measurements on four crystals. Seen in a microscope fitted with an excellent eyepiece goniometer, planes of polarization in the crystals are evidently parallel and perpendicular to the planes 1 0 0, 0 0 1; but whether a plane of polarization bisects the acute angle of the wedge, *i. e.* is parallel to 0 1 0 or to 1 0 0, or whether 1 0 0 is equally inclined to the planes forming the wedge—in short, whether the crystal is oblique or prismatic, it is very difficult to determine. The mineral frequently presents itself, moreover, in what appear to be twinned forms; but the angles between the planes 1 0 0 in the two individuals are not sufficiently concordant, as measured on different crystals, to justify a speculation on the symbols of a twin face.

Several analyses of Waringtonite concur in establishing its formula as $3Cu''H'_2O_2 + Cu''SO_4 + H'_2O$, as is seen by the following numbers:—

		Percentage as calculated.	Average found.
4 equivs. copper	=126·72	= 53·99	54·48
4 equivs. oxygen..........	= 32·	= 13·63	(calc. 13·756)
1 equiv. sulphuric anhydride	= 40·	= 17·04	16·73
4 equivs. water	= 36·	= 15·34	14·64
	234·72	=100·00	99·606

It also contains traces of lime, magnesia, and iron, and appears to be generally mixed with a small proportion of another mineral, which is probably Brochantite, as Brochantite occurs in distinct crystals on some of the specimens of Waringtonite.

Its specific gravity is 3·39 to 3·47.

Its hardness is 3 to 3·5, being harder than calcite, and about equal in hardness to celestine.

The entire difference of its crystallographic habit, the absence of the striation and marked prismatic forms so characteristic of Brochantite, its habitually paler colour, lower specific gravity (in Brochantite G=3·87 to 3·9), and hardness sufficiently distinguish it from that mineral. The mountain-green streak offers an available means of contrasting Waringtonite

and Brochantite with Atacamite, the streak of which is of a characteristic apple-green.

M. Pisani has published analyses of the two above-described minerals. In the former (possibly from having driven off part of the water in the preliminary desiccation of the mineral) he has found less water than I consider it really to contain, and he has consequently given to Langite the formula of Waringtonite.

The green mineral which he has analyzed and described as Brochantite seems, from his analysis, to have contained a slight admixture of the ferruginous matrix, and also differs from mine in the estimate of the water. I confined my preliminary desiccation to a careful treatment of the bruised mineral with dried and warm blotting-paper, as many hydrated minerals of this class yield up part of their water when long exposed to a perfectly dry air, or to a temperature of 100° C.

II. "Preliminary Notice on the Products of the Destructive Distillation of the Sulphobenzolates." By JOHN STENHOUSE, LL.D., F.R.S., &c. Received February 15, 1865.

The salt which I have hitherto chiefly employed is the sulphobenzolate of soda, $C_{12} H_5 Na 2SO_3$, which was prepared according to Mitscherlich's* directions, by precipitating crude sulphobenzolate of lime by carbonate of soda, separating the carbonate of lime produced, and evaporating the clear solution to dryness. The finely powdered salt, which had previously been thoroughly dried, was introduced into a small copper retort and subjected to destructive distillation, when a considerable quantity of carbonic acid was evolved, and a brownish-coloured oily liquid, covered by a layer of water, collected in the receiver.

This oil was separated from the water and distilled in a retort furnished with a thermometer. The liquid began to boil at 80° C., and then rose slowly to 110° C., when only a small quantity of water, and an oil consisting chiefly of benzol, came over. The boiling-point then rapidly rose to 290° C., at which temperature the greater portion of the liquid distilled over, leaving a black residue in the retort.

The oil boiling at 290° C. is of a pale yellow colour, heavier than water, and has an aromatic and slightly alliaceous odour. It contains a considerable amount of sulphur.

When this oil is brought in contact with nitric acid, a very violent action ensues with evolution of nitrous fumes, and when the resulting solution is poured into water, a crystalline mass of a pale yellow colour is obtained. This, when dried and washed with ether to separate a small quantity of adhering oil, is dissolved in hot spirit, from which, on cooling, two colourless crystalline substances separate.

The first of these, which constitutes the bulk of the product, forms beautiful rhombic plates, which, when crystallized out of benzol, may be

* Pogg. Ann. vol. xxxi. pp. 283 & 634.

obtained of considerable size and great lustre, closely resembling chlorate of potassa in appearance. This body also contains sulphur. The second substance, the quantity of which is comparatively small, crystallizes in long thin plates.

The oil, when treated with concentrated sulphuric acid, dissolves with a fine purple colour, and from this solution water precipitates a crystalline body, an organic acid remaining in solution, which forms a crystalline lime-salt.

I have likewise subjected to destructive distillation the sulphobenzolates of lime, ammonia, and copper. The two last yield very different products from the soda-salt.

I am at present engaged in examining these as well as the other bodies mentioned in this Notice, and hope soon to be able to communicate to the Society the results of my investigations.

III. "Preliminary Note on the Radiation from a Revolving Disk." By Balfour Stewart, M.A., F.R.S., and P. G. Tait, M.A. Received February 23, 1865.

The authors having been led by perfectly distinct trains of reasoning to identical views bearing on the dissipation of energy, have had preliminary experiments made on the increase of radiation from a wooden disk on account of its velocity of rotation, both in the open air and *in vacuo*.

These experiments were made with a very delicate thermo-electric pile and galvanometer. In the experiments in the open air the disk was of wood; its diameter was 9 inches, and it was made to rotate with a velocity somewhat less than 100 revolutions in one second.

A sensible effect was produced upon the indicating galvanometer when the disk was made to rotate, and this effect appeared to be due to radiation, and not to currents of air impinging against the pile. In amount it was found to be nearly the same as if the disk had increased in temperature $0^{\circ}\cdot 75$ Fahr.

In the experiments *in vacuo* the diameter of the wooden disk was over 12 inches; its velocity of rotation was about 100 revolutions in one second, and the pile was nearer it than when in air. Under these circumstances, with a vacuum of 0·6 in., an effect apparently due to radiant heat was obtained, amounting to nearly the same as if the disk had increased in temperature $1^{\circ}\cdot 5$ Fahr.

Bearing in mind the increased diameter of the disk, the effect is probably equivalent to that obtained in air, and these preliminary experiments would tend to show that when a wooden disk is made to revolve rapidly at the surface of the earth, its radiation is increased to an extent depending on the velocity; and it would appear that this effect is not materially less in a vacuum of 0·6 in. than in the open air.

The authors intend to work out this and allied questions experimentally, and hope, if successful, to communicate the result to this Society.

March 2, 1865.

Major-General SABINE, President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read, as follows:—

James Abernethy, Esq., C.E.
A. Leith Adams, M.B.
Alexander Armstrong, M.D.
William Baird, M.D.
George Bishop, Esq.
John Charles Bucknill, M.D.
Lieut.-Col. Cameron, R.E.
Henry Christy, Esq.
The Hon. James Cockle.
The Rev. William Rutter Dawes.
W. Boyd Dawkins, Esq.
Henry Dircks, Esq.
Thomas Rowe Edmonds, Esq.
Professor Henry Fawcett.
Peter Le Neve Foster, Esq.
Sir Charles Fox, C.E.
Archibald Geikie, Esq.
George Gore, Esq.
Professor Robert Grant.
George Robert Gray, Esq.
William Augustus Guy, M.B.
Capt. Robert Wolseley Haig, R.A.
George Harley, M.D.
Benjamin Hobson, M.B.
William Huggins, Esq.
Fleeming Jenkin, Esq., C.E.
Edmund C. Johnson, M.D.
Henry Letheby, M.B.
Professor Leone Levi.
Waller Augustus Lewis, M.B.
John Robinson M'Clean, Esq., C.E.
Capt. Sir F. Leopold M'Clintock, R.N.
Robert M'Donnell, M.D.
Hugo Müller, Esq., Ph.D.
Charles Murchison, M.D.
Andrew Noble, Esq., C.E.
Sir Joseph P. Olliffe, M.D.
William Kitchen Parker, Esq.
William Henry Perkin, Esq.
Thomas Lamb Phipson, Esq., Ph.D.
Charles Bland Radcliffe, M.D.
Lovell Reeve, Esq.
John Russell Reynolds, M.D.
Thomas Richardson, Esq., Ph.D.
Wm. Henry Leighton Russell, Esq.
Edward Henry Sieveking, M.D.
Alfred Tennyson, Esq., D.C.L.
George Henry Kendrick Thwaites, Esq.
The Rev. Henry Baker Tristram.
Lieut.-Col. James Thomas Walker, R.E.
A. T. Houghton Waters, M.D.
Charles Wye Williams, Esq.
Henry Worms, Esq.

The following communications were read:—

I. "On the Quadric Inversion of Plane Curves." By T. A. Hirst, F.R.S. Received February 16, 1865.

Introduction.

1. The method of inversion which forms the subject of the present paper is an immediate generalization of that now universally employed. In place of a fixed circle with the origin at its centre, any fixed conic (*quadric*)

whatever is taken, as a fundamental curve, and the origin is placed anywhere in its plane. In this manner many descriptive relations which in the ordinary theory are masked, regain the generality and prominence to which they are entitled. Having long ago convinced myself of the *utility* of this generalized method of inversion, I deem it desirable to establish, for the sake of future reference, its chief general principles. With the view of securing the greatest possible familiarity with the effects of inversion, I employ purely geometrical considerations, and everywhere give preference to a direct and immediate contemplation of the several geometrical forms which present themselves. The examples occasionally introduced, are given for the sake of illustration merely; they do not exhibit the full power of the method. Moreover, to prevent, as much as possible, the extension of a paper intended for publication in the *Proceedings of the Royal Society*, no attempt has been made to subject such special cases to exhaustive treatment. The figures are, for the most part, simple; the fundamental one being given, the rest may readily be drawn or imagined; when treating of the effects of inversion on the higher singularities of curves, however, I have thought it desirable to refer by the initials (𝔄. ℭ.) to articles and figures in Plücker's elaborate work on the *Theorie der Algebraischen Curven*. The relation which the present method bears to the still more general one of *quadric transformation*, as developed in 1832 by Steiner in his *Geometrische Gestalten*, and by Magnus in the eighth volume of Crelle's *Journal*, offers several points of interest to which I propose to return on a future occasion*.

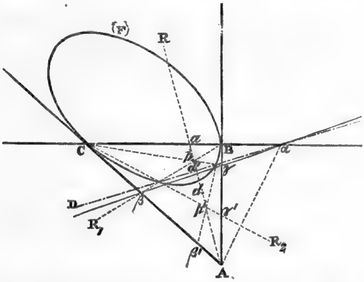
Definitions.

2. Two points, p and p', conjugate to each other with respect to a fixed *fundamental conic* (F), and likewise collinear with any fixed *origin* A in the plane of the latter, are said to be inverse to each other, relative to that conic and origin. In other words, the inverse of a point is the intersection of its polar, relative to (F), and the line which connects it with the origin A. From this the following property is at once deduced:

i. *The several pairs of inverse points* p, p′, *on any line* R *through the origin* A, *form an involution, the foci of which are the intersections, real or imaginary, of that line and the fundamental conic* (F).

* I have recently been interested to find that the method of quadric inversion was distinctly suggested, though never developed, by Prof. Bellavitis of Padua no less than twenty-seven years ago. Considering the date of its appearance, the memoir, in the last paragraph of which this suggestion was made, is in many respects a remarkable one. It is entitled *Saggio di Geometria Derivata*, and will be found in the fourth volume of the *Nuovi Saggi dell' I. R. Accad. di Scienze, Lettere ed Arti di Padova*. Two years previously, that is to say in 1836, the same geometer had developed, very fully, the principles of the ordinary method of *cyclic* inversion; which latter, after a lapse of seven years, appears to have been first proposed in England by Mr. J. W. Stubbs, B.A., Fellow of Trinity College, Dublin, in a paper "On the Application of a new Method to the Geometry of Curves and Curve Surfaces," published in the Philosophical Magazine, vol. xxiii. p. 338.

Two curves are said to be inverse to each other, of course, when the several points of one are inverse to those of the other. The latter is sometimes referred to as the *primitive*, and the former as its *inverse*; the relation between the two curves, however, is a mutual one, and the distinction is merely introduced for convenience. In order to obtain clear conceptions of the various relations which exist between inverse curves, it will be found convenient to place the origin A outside the fundamental conic (F). The modifications to be introduced when the origin is placed elsewhere are quite obvious, and, except in a few instances, need not be specially alluded to.



3. Adopting terms introduced by Magnus, the origin A and the two points of contact B and C of the tangents from A to the fundamental conic (F) are called the *principal points*; the triangle, of which they form the corners, is termed the *principal triangle*, and its sides BC, CA, AB, respectively *polar* to A, C, B, are the *principal lines*. Occasionally it will be convenient to refer to B and C as distinct from A, which is always real; the two former will then be called the *fundamental* points, and the principal lines AB, AC, which there touch (F), will in like manner be called the *fundamental* lines *polar* to B and C.

4. This premised, it is manifest from Art. 2 that, in general, a point p has but one inverse point p'. The only exceptions, in fact, are the three principal points, each of which is obviously inverse to every point in the principal line which constitutes its polar. It is further evident that each point of the fundamental conic (F) coincides with its own inverse, and that the several points of (F) are the only ones in the plane which possess this property.

Hence may be inferred, without difficulty, the following theorem:

i. *If any two curves have* r*-pointic contact with each other at a point* p, not on a principal line, *their inverse curves will also have* r*-pointic contact with each other at the inverse point* p′.

Relative orders of inverse Curves.

5. The order of the curve inverse to a given curve (P) of the order n

may readily be ascertained. For in the first place, since (P) has n points on each principal line, its inverse must pass n times through each principal point (Art. 4); and secondly, since a line R drawn through the origin A intersects (P) in n points, none of which are, in general, situated on the principal line BC, polar to A, the same line R will intersect the inverse curve, not only in the n points coincident with the origin, but also in n points distinct therefrom. Hence

The complete *quadric inverse of any curve of the order* n *is a curve of the order* 2n, *which has multiple points, of the order* n, *at each of the three principal points.*

6. The term *complete* is here used because, under certain circumstances, the inverse curve will break up into one or more of the principal lines, each taken once or oftener, and a *proper* inverse curve (P′) of lower order, and which passes less frequently through the principal points. This, by Art. 4, will be the case whenever the primitive curve (P) passes through a principal point; and it is obvious that the order of (P′) will then be less than $2n$ by the total number of such passages. Further, the multiplicity of any principal point on (P′) will be less than n by the number of times the two principal lines, which there intersect, enter into the complete inverse,—in other words by the number of passages of the primitive curve (P) through the poles of those principal lines.

Hence, if a, b, c denote, respectively, the multiplicities of the principal points A, B, C on the curve (P), and if a', b', c' have the same significations relative to the inverse curve (P′) of the order n', we shall have

$$\begin{aligned} n' &= 2n-a-b-c, \\ a' &= n-b-c, \\ b' &= n-a-b, \\ c' &= n-a-c. \end{aligned}$$

These equations, by transformation, may readily be made identical with those which result therefrom by simply interchanging the accented and like non-accented letters; this shows, of course, that between proper inverse curves the same *mutual* relation exists as between inverse points. It is in virtue of this mutual relation that the theorems to be given hereafter are all conversely true; the enunciations of the converse theorems may therefore, in all cases, be omitted.

7. The above equations also furnish the following relations:

$$n'-(a'+b'+c')=(a+b+c)-n=n-n',$$
$$n'-a'=n-a,\ n'-b'=n-c,\ n'-c'=n-b,$$

from which we learn that

i. *The difference between the orders of two inverse curves is numerically the same as that between the order of either, and the total number of its passages through the three principal points.* This latter difference, however, has opposite signs for the two curves.

ii. *For a curve to be of the same order as its inverse, and to pass the*

same number of times through each of the three principal points, it is necessary and sufficient, first, *that its order be equal to the total number of its passages through the three principal points*; *and* secondly, *that it pass as frequently through one fundamental point as through the other.*

It would be easy, by the second theorem, to determine the number and nature of the several curves of a given order which have inverse curves of the same order and like properties, relative to the principal points. This determination, however, as well as the solution of the allied question—

Under what conditions will a curve of given order coincide with its own inverse?

will more appropriately form the subject of a separate paper. It will be sufficient to note here that a right line through the origin, and the fundamental conic itself, regarded as a primitive curve, are the simplest instances of the kind under consideration. Another example is also alluded to in Art. 11. Ex. 3.

Conics inverse to Right Lines.

8. From Arts. 2 and 6, as well as directly from elementary geometrical principles*, it follows that

i. *The inverse of a right line is, in general, a conic passing through the three principal points and the two intersections of the fundamental conic and the primitive line, as well as through the pole of the latter, relative to the former.*

It is only when the primitive line passes through a principal point that the inverse conic breaks up into the fixed principal line, polar to that point, and another right line (the proper inverse) through the principal point *opposite* to that fixed principal line (Art. 4). Thus:

ii. *The proper inverse of a right line passing through one of the two fundamental points is a right line passing through the other, and these inverse right lines always intersect on the fundamental conic.*

The following is an immediate consequence of this and the theorem i. of Art. 4:

iii. *If one of two inverse curves have* r*-pointic contact,* not on a principal line, *with a right line passing through a fundamental point, the other will have* r*-pointic contact with the inverse line, through the other fundamental point, and the points of contact, being inverse to each other, will be collinear with the origin.*

The modification which this theorem suffers when one of the points of contact is on a principal line, will shortly be fully considered.

9. The line at infinity has also its inverse conic (I), which is of importance in many inquiries connected with inversion. On observing (Art. 2) that the inverse of the infinitely distant point of any line R through

* An elegant demonstration of this theorem, identical with the elementary one alluded to, has been inserted by M. Chasles in Art. 209 of his excellent *Traité des Sections Coniques*, the first part of which has just reached me.

the origin is the middle point of the segment which (F) intercepts on R, and that (Art. 4) the infinitely distant points of (F) are necessarily also on (I), it will be seen that

i. *The conic* (I), *circumscribed to the principal triangle, which is inverse to the line at infinity, is similar and similarly situated to the fundamental conic, of which latter, in fact, it bisects all chords that converge to the origin.*

By means of this conic (I) the asymptotes to any inverse curve may be readily constructed. Since the next article, however, will be devoted to the construction of the tangent at any point whatever of an inverse curve, it will be sufficient here to note the following obvious corollaries of the above theorem:

ii. *The asymptotes of either of two inverse curves are respectively parallel to the right lines connecting the origin with the intersections of the other curve and the conic* (I), *which circumscribes the principal triangle and is, at the same time, similar and similarly situated to the fundamental conic* (F).

iii. *The conic inverse to a given right line will be a hyperbola, a parabola, or an ellipse, according as that line cuts, touches, or does not meet the conic* (I), *which circumscribes the primitive triangle and is similar and similarly placed to the fundamental conic* (F).

The conic (F) and the circle circumscribed to the principal triangle ABC have, of course, conjugate to BC, a second common chord H, inverse to that circle (Art. 8, i.), and this chord is clearly the only line in the plane whose inverse is a circle. The imaginary intersections of H with (I) are *inverse to the circular points at infinity*, and consequently lie, with the latter, on a pair of imaginary lines intersecting in the origin A.

Again, it is well known that all chords of (I) which subtend a right angle at A pass through a fixed point h *. The conics inverse to such chords are readily seen to be equilateral hyperbolas, and like their primitive lines they all pass through a fixed point—in fact, through the point h' inverse to h; this point h', moreover, is well known to be the intersection of the three perpendiculars of the triangle ABC, about which all the equilateral hyperbolas are circumscribed †. It further follows from a known theorem, that the chords of (I) which subtend a constant angle at A envelope a conic which has double contact with (I) at the inverse circular points, or, in other words, at the imaginary intersections of (I) and H; and conversely that all tangents to such a conic intercept on (I) an arc which subtends at A a constant angle ‡. The conics inverse to such tangents are, when the latter actually cut (I), hyperbolas whose asymptotes are inclined to each other at a constant angle—that is to say (in order to embrace all cases), *similar*

* Salmon's 'Conic Sections,' 4th ed., Art. 181, Ex. 2.

† *Ibid.* Art. 228, Ex. 1. Two distinct theorems in conics are thus brought, by inversion, into juxtaposition, and we have a simple example of the *duality* which this method, like that of reciprocal polars, imparts to every theorem.

‡ Chasles's *Sections Coniques*, Arts. 473, 474; also Salmon's 'Conic Sections,' Arts. 276, 277, 296.

conics. Now, by Arts. 4 and 6, the inverse of a conic which has double contact with (I) at the inverse circular points is, in general, *a quartic curve having double points at* A, B, C, *and touching, at the circular points, the line at infinity.* This curve, therefore, is also the *envelope of similar conics circumscribed to the triangle* ABC *. The point h clearly belongs to the above series of conics having double contact with (I), and H must be its polar relative to (I) †; so that we may resume as follows:

iv. *The right lines whose inverse conics are equilateral hyperbolas, all pass through the point* h *which is inverse to the intersection of the three perpendiculars of the principal triangle; the circle circumscribed to this triangle is the inverse of the polar* H *of the point* h, *relative to the conic* (I) *which is inverse to the line at infinity; the imaginary intersections of* H *and* (I) *are the* inverse *circular points, and all lines which envelope a conic* (Σ) *having double contact at these points with the conic* (I) *are inverse to conics which are similar to each other.*

Tangents to inverse curves at inverse points.

10. To a pencil of right lines L, whose centre is p, corresponds, by quadric inversion, a pencil of conics (L′) passing through the three principal points (Art. 8, i.) and the inverse point p'. To each element of the one pencil corresponds manifestly but one element of the other; so that the lines L through p, and the tangents L′, at p', to their respective inverse conics (L′) constitute two homographic pencils ‡; and since two corresponding rays of the latter coincide with pp', every other pair must intersect on a fixed line D (see figure). Since, moreover, to the rays p B, p C correspond, respectively, the rays p' C, p' B (Art. 8, ii.), it is obvious that the line D is simply one of the three diagonals of the complete quadrilateral p B p'C, the other two diagonals being pp' and BC. Hence if a be the intersection of the latter, the former D will, in virtue of a well-known property of the quadrilateral, pass through the harmonic conjugates d' and α of a, relative respectively to pp' and BC. Now α is at once recognized to be the pole, relative to the fundamental conic, of pp' or R; so that d, the inverse

* From this a new definition may be readily deduced of the interesting curve, of the fourth order and third class (with three cusps, and the circular points for points of contact of an infinitely distant double tangent), which presented itself to Steiner as the envelope of the line passing through the feet of the perpendiculars let fall from any point of a circle upon the sides of an inscribed triangle, and of which he has enunciated (merely) so many remarkable properties in a paper published in vol. liii. of Crelle's *Journal.* The curve is, in fact, *the envelope of an hyperbola circumscribed to an equilateral triangle, and having its asymptotes inclined to each other as are any two sides of that triangle.* The curve may also be generated as a hypocycloid, and appears to be identical with the one whose equation is given at p. 214 of Dr. Salmon's 'Higher Plane Curves.'

† Chasles's *Sections Coniques*, Art. 474.

‡ Chasles, "*Principe de correspondance entre deux objets variables,*" &c., *Comptes Rendus*, Dec. 24, 1855, and *Sections Coniques*, Art. 325. See also Cremona's '*Teoria geometrica delle curve piane,*' p. 7.

of d', must be the pole of D or ad', as well as the harmonic conjugate of A relative to pp' (Art. 2, i.). Consequently, from the fact that when L touches, at p, a primitive curve (P), the conic (L'), and hence its tangent L', must touch, at p', the inverse curve (P') (Art. 4, i.), we at once deduce the following theorem, by means of which the tangent at any point of a curve inverse to a given one may, for all positions of the origin, be readily constructed:

i. *The tangents, at two inverse points, to two inverse curves intersect on the polar, relative to the fundamental conic, of the harmonic conjugate of the origin with respect to their points of contact.*

Hence we may also deduce the following property:

ii. *To a multiple point on one of two inverse curves,* but not on a principal line, *corresponds, on the other, a multiple point of the same order of multiplicity, and the tangents to corresponding (inverse) branches all intersect on the polar, relative to the fundamental conic, of the harmonic conjugate of the origin, relative to the two multiple points.*

The reality, taction, and general distribution of the several branches will be the same at two such multiple points; the latter, in fact, will merely differ in certain secondary properties. For instance, a branch inflected at one of these points would not, in general, correspond to an inflected branch at the other; the latter branch, however, would have three-pointic contact, at this multiple point, with the conic inverse to the tangent of the inflected branch at the first multiple point.

11. The tangents at the principal points to two inverse curves may be thus investigated.

Exclusive of the principal points B and C, let the primitive curve (P) intersect the principal line BC in the points α, α_1, α_2, &c. . ., and conceive a right line R to rotate around the origin A. Exclusive of A this line R will intersect (P) in $n-a$ points p, respectively inverse to the $n'-a'$ points p' in which it intersects the inverse curve (P') (Art. 7). It is clear, however, that whenever, by the rotation of R, p approaches one of the points α, p' will approach to coincidence with A, so that R will there touch a branch of (P'); and more generally, that if R should have $(r-1)$-pointic contact at α with (P) it would, at the same time, have r-pointic contact at A with one of the branches of (P').

Similarly, if (P) intersect the fundamental line AC in the points β, β_1, β_2, &c. ., A and C being excluded, and a right line R_1, turning around B, intersect (P) in $n-b$ points p, their $n'-c'$ inverse points p' (Art. 7) will be the intersections of (P') and the line R_2, inverse to R_1 and passing through C (Art. 8, ii.). Each pair of points p, p', moreover, will be collinear with A (Art. 2). Hence it follows that whenever, by the rotation of R, two points p and β approach each other, the inverse point p' will approach C, so that the line R_2 will there touch a branch of (P); and, as before, if R_1 have $(r-1)$-pointic contact with (P) at a point β, R_2 will have r-pointic contact at C with a branch of (P').

In a similar manner the line R_2, which connects C and one of the intersections γ of AB and (P), has for its inverse a line R_1 touching, at B, a branch of (P′). All these cases are included in the following theorems:—

i. *The tangents at a principal point to one of two inverse curves are respectively inverse to the right lines which connect the intersections of the other curve and the principal line polar to that point, with the opposite principal point.*

ii. *If a non-principal line have* r*-pointic contact at a principal point with any branch of one of two inverse curves, the inverse line will have* $(r-1)$*-pointic contact with the other curve on the principal line polar to that principal point.*

The second of these theorems, as will be shown in the next article, is slightly modified when the line of r-pointic contact is a principal one; the first theorem, though still true, becomes susceptible of the following simpler enunciation:

iii. *If a branch of one of two inverse curves touch a principal line at a principal point, the other curve will have a branch touched by the polar of this point at the pole of that line.*

The following examples will serve as illustrations of these three theorems:

Ex. 1. The primitive being a right line intersecting the principal lines in α, β, γ, respectively (see figure), Aα will be the tangent at A to its inverse conic; and if Bβ, Cγ intersect in p, the inverse point p' will be the pole of BC relative to the inverse conic. This pole is always real; it may, moreover, be easily constructed, even when B and C are imaginary, on observing that p is also the intersection of the polar of α, relative to the fundamental conic, with the harmonic conjugate of αA, relative to BC and the primitive line.

Ex. 2. The primitive being a conic passing through the origin and intersecting the fundamental lines in β and γ, its inverse will be a cubic passing through B and C, and having a double point at A (Art. 6). This latter point will be a node, if the primitive conic cut BC in real points α_1, α_2; and Aα_1, Aα_2 will be the tangents thereat. It will be a conjugate or isolated point, however, when the primitive conic does not actually cut BC. The tangents to the cubic at B and C will, as before, intersect in the point p', inverse to the intersection p of Bβ, Cγ. If the primitive conic touch the latter lines in β and γ, in which case it is manifest that it cannot cut BC, then the cubic will have *real points of inflection at* B *and* C, *and, necessarily, a conjugate point at* A*. The line Ap is obviously the polar, relative to the primitive, as well as to the fundamental conic, of the intersection α of the lines BC, $\beta\gamma$; hence αA touches the primitive conic at A, and, by i., α is a point on the cubic; it is, in fact, the third point of inflection on this curve.

Ex. 3. The primitive being a conic touching the fundamental lines in

* The truth of a well-known theorem in cubics, 'Higher Plane Curves' (Art. 183), is here rendered visible.

the fundamental points, the inverse curve will be another conic possessing the same properties (Art. 6). The fundamental conic is not the only one of such a series which coincides with its own inverse (Art. 7); for there is obviously a second one which cuts every line through the origin in a pair of inverse points.

Singularities of inverse Curves.

12. If two of the intersections $\alpha, \alpha_1, \alpha_2$ &c. .. of the primitive curve (P) with the principal line BC coincide; in other words, if (P) touch BC at α; then two of the branches of (P′) will unite to form a cusp at A, at which Aα will be the tangent. Similarly, if (P) touch a fundamental line, say AC at β, then on one of the branches of (P′) there will be a cusp at C, at which the line inverse to Bβ will be the tangent. In short—

i. *If one of two inverse curves touch a principal line at a non-principal point, the inverse of the connector of the point of contact with the opposite principal point will touch, at the pole of that line, a cusped branch of the other curve.*

The more general theorem is obviously this:

ii. *If one of two inverse curves have* r*-pointic contact with a principal line at a non-principal point,* r *branches of the other curve will coalesce so as to form a branch on which there will be a multiple point of the* r*th order at the pole of that line; and the sole tangent to this branch will be the inverse of the connector of the point of contact of the first curve with the opposite principal point.*

It may be added that, in general, the singularity at the principal point on this branch will be invisible or cusp-like, according as r is odd or even. Thus:

Ex. 1. If the primitive conic considered in Art. 11, Ex. 2, not only pass through the origin, but touch the principal line BC in α, its inverse cubic will, besides passing through B and C, have a cusp at A, the tangent at which will be Aα.

Ex. 2. If the primitive curve be a cubic with a node; then, the latter being taken as origin, the tangents thereat as fundamental lines, and the real stationary tangent of the cubic as the third principal line; the inverse curve will be a quartic which touches the latter line at the fundamental points B and C (Arts. 6 and 11, iii.), and has moreover a triple point at A, at which the sole tangent passes through the point of inflection on the primitive cubic. This tangent meets the quartic in four points coincident with A (𝔄. 𝔈. p. 190)*.

13. If the r-pointic contact in the preceding theorem occur at a principal point, we may conceive it to have arisen from the approach thereto of a point on the principal line, where the contact was $(r-1)$-pointic; the unique tangent to the branch of the inverse curve upon which there is a multiple point of the order $(r-1)$ will also, by this approach, have become coincident with a principal line †. Hence—

* See also Salmon's 'Higher Plane Curves,' Art. 217.

† The case corresponding to $r=2$ has already been considered in Art. 11, iii.

i. *If a branch of one of two inverse curves have* r*-pointic contact with a principal line at a principal point, the other curve will have, at the pole of that line, a multiple point of the order* r—1 *on a branch the sole tangent to which is the polar of that principal point.*

Ex. The primitive curve being a cubic which has the fundamental points B, C for points of inflection, and the fundamental lines for stationary tangents, and consequently another point of inflection α on BC, the inverse curve will be a quartic touching Aα at the origin, and passing twice through each of the fundamental points (Arts. 6 and 10). From the present theorem we conclude, further, that the latter points will be cusps on the quartic, and that the fundamental lines will be the tangents thereat (𝔄. 𝔆. p. 192, ix.).

14. From preceding articles the following properties may also be deduced:

i. *If one of two inverse curves have a multiple point on a principal line,* but not at a principal point, *the other will, in general, have a corresponding number of branches touching each other at the pole of that line; and at this pole the common tangent to these branches will be inverse to the connector of the first multiple point and the principal point opposite to the principal line on which it is situated.*

To obtain a clear conception of the modifications which may present themselves, it will suffice to consider the case where *the primitive curve* (P) *has a double point at* α *on the polar* BC *of the origin* A.

(a) The inverse curve will, in general, have a *tacnode* at A (𝔄. 𝔆. p. 164, figs. 17, 18)—in other words, two branches which there touch each other, the common tangent being Aα; these branches will, moreover, have three-pointic contact, at A, with the conics inverse to the two tangents at α to the primitive curve*. If one of the tangents at α coincide with αA, one of the branches of the inverse curve will be inflected at A (𝔄. 𝔆. fig. 21) (Art. 11, iv.). If one of the branches at α touch the primitive line BC, then one of the branches at A will be cusped, Aα being still the common tangent to the cusped and to the ordinary branch (𝔄. 𝔆. fig. 28). If both these singularities occur on the primitive at the same time, the inverse curve will present at A a triple point, with a single tangent, formed by an inflected and a cusped branch (𝔄. 𝔆. fig. 30).

(b) If the tangents at α coincide, so that the branches of the primitive curve there form an ordinary or *ceratoid* cusp (𝔄. 𝔆. fig. 16, *Spitze erster Art*), the conics of three-pointic contact at A with the corresponding branches of the inverse curve will also coincide, and the latter will possess a *ramphoid cusp* † (𝔄. 𝔆. fig. 19, *Spitze zweiter Art*), at which Aα will still be the sole tangent, meeting the cusp in *four* coincident points. In the special case where the tangent at the cusp α passes through the origin, the

* By Art. 11, i., the conics inverse to any right line whatever through α will have two-pointic contact, at A, with each branch of the tacnode.

† Prof. Cayley's term *cusp-node* is more appropriate (Quart. Journ. vol. vi. p. 74); the singularity in question may also be regarded as a stationary point on a stationary tangent, for the curve lies entirely on one side of the latter.

cusp at A on the inverse curve will also assume the ceratoid form, but it will be of higher order than the primitive one, since the tangent at A will meet it in *five*, instead of in *three* coincident points (𝔄. 𝔈. p. 167, iv.). If the principal line BC be itself the tangent at the ordinary cusp α, the inverse curve will have a triple point at A, the sole tangent $A\alpha$ at which will meet the curve in *five* coincident points. To the eye, this singularity will have the form of a point of inflection (𝔄. 𝔈. p. 174, fig. 29).

The following examples will illustrate the production, by inversion, of cusps of both kinds.

Ex. 1. The primitive curve being a cusped cubic, and the origin A being placed at its real point of inflection, let the stationary tangent be chosen as a fundamental line, and any line whatever through the cusp α as the polar of the origin. If the points B, C, in which the latter intersects the cubic and the stationary tangent, be considered as the fundamental points, the inverse curve will (Art. 6) be a quartic curve passing once through B, and twice through each of the points C and A. The tangent to the quartic at B will be the inverse of the line joining C to the third intersection γ of the cubic with the fundamental line AB (Art. 11, i.). The point C will be a *ceratoid cusp* on the quartic with CB for its tangent (Art. 13, i.). Lastly, $A\alpha$ will be the tangent at A to a *ramphoid cusp* on the inverse curve (b). The latter, therefore, is identical with the very remarkable quartic curve to which Prof. Sylvester's recent researches on the roots of equations of the fifth degree has imparted so great an interest (Phil. Trans. 1865). It is termed by him the 'Bicorn,' and is the one which, in Plücker's classification, is numbered xvi. (𝔄. 𝔈. p. 193). Since the primitive cubic from which it has been derived may itself be regarded as the inverse of a conic (Art. 12, Ex. 1), it is obvious that many properties of the Bicorn may be deduced from those of a conic, by double inversion, relative to two sets of principal points.

Ex. 2. The primitive being a conic, its inverse will, in general, be a quartic curve passing twice through each principal point (Art. 6).

All the ten varieties of such quartics which have been described by Plücker (𝔄. 𝔈. p. 195) correspond, in a very simple manner, to the different positions which the primitive conic may have*. Now it is well known† that, in general, two triangles may be inscribed in this conic, each of which shall, at the same time, be circumscribed to the principal triangle; whence we infer that two triangles may be inscribed in the quartic, so that a double point shall lie on each side of each triangle. A second inversion, therefore,

* For instance, if the principal triangle be self-conjugate relative to the primitive conic, the inverse quartic will have, at each principal point, *both its branches inflected* (Art. 11, ii.). In this case it is further obvious that two principal points must necessarily lie *outside* the primitive conic; so that *one* principal point will necessarily be a conjugate point on the quartic (Art. 11, Ex. 2). Inversion, in fact, renders visible the many curious properties, signalized by Plücker, which quartic curves present whenever their double points are also points of inflexion (𝔄. 𝔈. p. 199).

† Salmon's Conic Sections, 4th ed. p. 237; Chasles's *Sections Coniques*, Art. 246.

relative to either of these triangles will transform the quartic to a quintic (Art. 6) with three tacnodes, the varieties of which will correspond to those of the quartic. If, for instance, the primitive conic were inscribed in the original principal triangle, then the quartic would have three *ceratoid cusps,* and the quintic would be the remarkable one which Plücker has signalized (𝔄. 𝔈. p. 222, fig. 35) as possessing three *ramphoid cusps.*

(c) With respect to singularities of a higher order on the primitive curve, and on a principal line, little more need be added. To a tacnode at α would correspond, on the inverse curve, two branches touching the line $A\alpha$, and having three-pointic contact with each other at A (𝔄. 𝔈. p. 165, fig. 20). In fact, as a general rule, the contact of the branches at A is one higher in order than that of the corresponding branches at α. If α were a ramphoid cusp on the primitive, A would also be a ramphoid cusp, of higher order, on the inverse curve (𝔄. 𝔈. p. 170), and so on. It is worth observing, lastly, that although, by (a), the inverse of a tacnode at A is an ordinary node at α, on the polar of A, the latter will itself become a tacnode when approaches to coincidence with B or C (Art. 11, iii.), and similarly—

i. *If one of two inverse curves have a ramphoid cusp at a principal point, to which a principal line is the tangent, the other will also have a ramphoid cusp at the pole of that line, the tangent to which will be the polar of that point.*

This is manifest, in fact, from (b), on considering, with Professor Cayley *, a ramphoid cusp at B, with tangent BC, to arise from the coincidence of a ceratoid cusp at α with a node at B.

Special cases of quadric inversion.

15. The special cases of inversion which correspond to particular hypotheses relative to the position of the origin and to the nature of the fundamental conic are very numerous. The choice of these elements will depend of course upon the nature of the properties which are to be investigated by the method of inversion. A few only of the more useful of such special cases can be here noticed.

(1) *When the fundamental conic* (F) *is an hyperbola with its centre at the origin* A, its asymptotes constitute the fundamental lines, and their intersections with the line at infinity the fundamental points (Art. 3). Every right line parallel to one of these asymptotes has, for its inverse, a right line parallel to the other; and the two intersect on the fundamental hyperbola (Art. 8, ii.). The inverse of every other line in the plane is an hyperbola passing through the origin, and having its asymptotes parallel to those of the fundamental hyperbola (Art. 8, i.); moreover the conic (I) inverse to the line at infinity resolves itself into these asymptotes (Art. 9, i.). Every hyperbola which does not pass through the origin, but has its asymptotes parallel to those of the fundamental one, has, for its inverse, an hyperbola

* Quarterly Journal of Mathematics, vol. vi. p. 74.

possessing the same properties (Art. 6); and if the primitive have likewise its centre at the origin, the latter will also be the centre of its inverse (Art. 11, Ex. 3). The tangents to two inverse curves at two inverse points p, p' now intersect on a line D *bisecting* pp', and parallel to the diameter of the fundamental conic, which is conjugate to pp' (Art. 10).

(1*a*) *When the fundamental conic is an equilateral hyperbola, the origin being still at its centre,* the method of inversion becomes identical with the "hyperbolic transformation" investigated by Professor Schiaparelli, of Milan, in his interesting memoir, "*Sulla trasformazione geometrica delle figure*" *. The line D, upon which the tangents to two inverse curves at inverse points p, p' intersect, not only bisects pp', but is now inclined at the same angle as pp' to each of the fixed asymptotes of the fundamental hyperbola.

(2) *The fundamental conic being an ellipse and the origin at its centre,* the inverse of every right line in the plane will be an ellipse passing through the origin, and at the same time similar, as well as similarly placed, to the fundamental ellipse. The ellipse (I) inverse to the line at infinity now resolves itself to a *point* coincident with the origin. Every ellipse not passing through the origin, but similar and similarly placed to the fundamental one, has for its inverse an ellipse with the same properties; and should the primitive be likewise concentric with the fundamental ellipse, so also will be the inverse. The tangents at two inverse points p, p' to inverse curves again intersect on a line D which bisects pp', and is parallel to the diameter of the fundamental ellipse conjugate to pp'.

(2*a*) *When the fundamental conic is a circle with its centre at the origin,* we have, as already stated, the case of *cyclic* inversion, and the imaginary circular points at infinity are the fundamental points; the line D becomes, as is well known, the perpendicular to pp' through its middle point. From the theorems ii. and iii. of Art. 8 we should now infer that

(i) *The cyclic inverse of a right line through one of the circular points is a right line through the other, and the two intersect on the fundamental circle.*

(ii) *The cyclic inverse of a* simple *focus of any curve is always a focus of the inverse curve—unless the first focus should coincide with the origin, in which case the inverse curve would have cusps at the circular points at infinity* (Art. 12, i.).

It is important to notice that this theorem does not hold for a *double* focus f of the primitive curve—that is to say, for the intersection of the tangents to this curve at the circular points. For in this case the connectors of the inverse point f' with the circular points would merely *intersect* the inverse curve on the fundamental lines (Art. 11, i.); the line joining the points of intersection—a common chord of the *point-circle* f' and the inverse curve—would, however, bisect Af' perpendicularly. Thus it is that the cyclic inverse of the centre f of a primitive circle is not a

* *Memoria della Reale Accademia delle Scienze di Torino*, Serie II. tom. xxi.

centre of the inverse circle, but the inverse of the origin relative to the latter circle. If the primitive curve had points of inflection at the circular points, the inverse f' of the intersection f (a triple focus) of the stationary tangents thereat would not only be a focus of the inverse curve, but its corresponding directrix would bisect Af' perpendicularly. The focal relations of cyclic inverse curves, however, deserve closer examination; and I propose on another occasion to return to them.

(3) *When the fundamental conic consists of a pair of real, right lines* F_1, F_2 *intersecting at* F, the fundamental points B, C coincide with F, and the principal line BC with the harmonic conjugate of AF relative to F_1, F_2. The conic (I) inverse to the line at infinity is now an hyperbola, of which AF is a diameter, and the asymptotes of which are parallel to F_1 and F_2. Harmonic conjugates relative to the latter lines now constitute pairs of inverse lines, and the conic inverse to every other line, cutting BC say in α, is a conic touched at A and F by $A\alpha$ and BC, so that the conics inverse to all lines parallel to BC are concentric, and have AF for a common diameter. The conics inverse to all lines passing through a fixed point a of AF have obviously three-pointic contact with each other at F, so that the conics inverse to lines parallel to AF have, at F, three-pointic contact with the hyperbola (I). All conics touching BC at F, but not passing through A, are inverse to conics having the same properties, and all conics passing through A and F, but not touching BC at the latter point, give by inversion conics of a similar description. The tangents at inverse points p, p' to two inverse curves now intersect on the harmonic conjugate of BC, relative to Fp, Fp'.

(3*a*) *The fundamental conic may consist of a pair of right lines, perpendicular to each other.* The results are then similar to, and somewhat simpler than those just noticed.

(4) *When the fundamental conic consists of a pair of imaginary right lines intersecting at a real point* F, the effects of inversion are analogous to those considered in (3). To secure real constructions, it is merely necessary to assume the *point-ellipse* F to be concentric with, similar, and similarly placed to a given ellipse (E). The lines FA and BC will then be conjugate diameters of (E), and any two inverse points p, p' will also lie on conjugate diameters. When the imaginary lines F_1, F_2 pass through the circular points, we have the following species of inversion:

(4*a*) *The fundamental conic consists of a point-circle* F. The principal line BC is now perpendicular to AF at F, and the connector of every pair of inverse points p, p' subtends a right angle at F. Inverse right lines through F, are perpendicular to each other, and the inverse of any right line in the plane is a conic, through A, to which AF is the normal at F. The circle (I) on AF as diameter, is the inverse of the line at infinity, and all right lines which touch one and the same circle, concentric with (I), are inverse to conics which are similar to each other—these conics being ellipses, parabolas, or hyperbolas, according as the circle in question is

greater than, equal to, or less than (I) (Art. 9, iii.). Lastly, every circle through F whose centre is on AF is inverse to a circle of the same kind as itself, as is also every circle passing through the points A and F.

(5) *When the origin* A *is on the fundamental conic* (F) the fundamental points B and C coincide with it, and the three principal lines coincide with the tangent at A. The conic (I) inverse to the line at infinity touches (F) at A and bisects all its chords through A, and the conic inverse to every other line in the plane has obviously three-pointic contact at A with (F). All lines which converge to one and the same point on the tangent at A are inverse to conics which have, at A, four-pointic contact with each other; hence right lines parallel to the tangent at A are inverse to conics having, at A, four-pointic contact with (I). Finally, the tangents to any two inverse curves, at inverse points p, p', intersect on a line D which touches (F) at the intersection of this conic and pp'.

A still more special case, into the details of which we cannot enter, is *when the fundamental curve is a parabola, and the origin at the infinitely distant extremity of its axis.* Inversion relative to a *conic and its focus* is also a special case which merits attention, but cannot be here considered.

Transformation correlative to quadric inversion.

16. Corresponding to the method of quadric inversion, there is of course a correlative method, which in certain inquiries is equally useful. It does not, however, require a separate exposition. It may also be remarked that the reciprocal polar, relative to the fundamental conic, of the inverse of any primitive curve, and the inverse of its reciprocal polar, lead at once to derivative curves, of which negative and positive Pedal Curves are special cases.

II. "On the Marsupial Pouches, Mammary Glands, and Mammary Fœtus of the *Echidna Hystrix*." By Professor Owen, F.R.S. Received February 18, 1865.

(Abstract.)

In a former communication on the generative economy of the Monotremata*, the author showed that the ovum left the ovarium with a spherical vitellus $1\frac{1}{2}$ line in diameter, and had attained a diameter of $3\frac{1}{2}$ lines in the uterus, the increase of size being due to increase of fluid between the chorion and vitelline tunics. This fluid, homologous with the albumen of the egg of oviparous vertebrates, did not coagulate in alcohol, and the only change presented by the vitellus of the largest observed ovum was a separation from the "food-yolk" of a "germ-yolk" in the form of a stratum of very minute granules, adhering to part of the membrana vitelli. There was no

* "On the Ova of the *Ornithorhynchus paradoxus*," Philosophical Transactions, vol. cxxiv. p. 555

trace of decidua in such impregnated uteri; the smooth chorion was firmer than that of uterine ova of Rodentia; whence, and for other reasons given in the Paper above cited, it was inferred "that the *Monotremata* were essentially ovo-viviparous."

The impregnated uteri of the Ornithorhynchus there described were of females killed in the month of October. In the early part of December young Ornithorhynchi, obtained from the nest, were transmitted to the author: they were naked, blind, with short, broad, flexible, and softly labiate mandibles, the tongue proportionally large, and reaching to near the end of the mandibles; the mouth not round, as in the mammary fœtus of Marsupials, but a wide transverse slit; a pair of small patulous nostrils opened upon the upper mandible, and between them was a small prominence resembling the knob on the beak of the newly-hatched chick, but softer, and lacking the cuticle, which had been torn off. There was no trace of navel or umbilical cicatrix.

The phases of the development of the mammary glands of the *Ornithorhynchus* were the subject of another communication, and, with the peculiar formation of the mouth of the young animal, demonstrated that it was nourished by milk as other mammals. The smallest of the young of the Ornithorhynchus so obtained did not exceed two inches in length.

At the early part of the present year, the author received from Dr. Mueller, F.R.S., of the Botanical Gardens, Melbourne, Australia, a female Echidna (*Ornithorhynchus Hystrix*, Home, *Echidna Hystrix*, Cuv.), with a young one, which the captor found adhering to the mother, as he supposed, by a nipple. They were transmitted in spirits, and their description forms the chief subject of the present communication. In regard to the parent, the description is limited to the parts concerned in generation.

The marsupial pouches are first described. These are two in number, about 1½ inch apart, each with the aperture longitudinal and towards the medial line, on the ventral integument, half an inch in depth and two-thirds of an inch in length. The young Echidna, about one inch in length in a straight line, could be received in a bent posture into the pouch, and might cling to the fine hairs of that part by its claws; but there was no trace of nipple. Each mammary gland terminates by numerous ducts upon the fundus of the corresponding pouch. The structure of the gland, the ducts, the surrounding muscles, and the pouch is described. The author next proceeds to give an account of the internal organs of generation of the female *Echidna*.

The left ovarium, as in the *Ornithorhynchus paradoxus*, was of an oblong flattened form, developed from the posterior division of the ovarian ligament and corresponding wall of the ovarian capsule; it consisted of a rather lax stroma, invested by a smooth, thin, firm "tunica propria," which glistens where stretched over the enlarged ovisacs. Of these there were five, of a spherical form, most of them suspended by a contracted part of their periphery, not stretched into a pedicle, to the rest of the

ovarium; the largest with a diameter of $1\frac{1}{2}$ line, the least of the five with a diameter of rather less than 1 line. Besides these there was a flattened ovisac, $2\frac{1}{2}$ lines in length, and 2 lines in opposite diameters, of a flattened pyriform shape, with a somewhat wrinkled exterior, attached by the base, with the apex slightly tumid, and showing a trace of a fine cicatrix. This was an ovisac from which an ovarian ovum had been discharged.

The oviducal branch of the ovarian ligament passes, as in the *Ornithorhynchus,* to the outer angle of the wide oviducal slit or aperture, which occupies or forms the margin of the ovarian pouch opposite to that to which the ovary is attached. The ligament spreads upon the inner wall of the infundibular part of the oviduct, and rejoins the ovarian division of the ligament to be continued along the oviduct, puckering up its short convolutions into a small compass. The "fallopian" aperture of the infundibulum is a longitudinal slit of 9 lines in length, with a delicate membranous border extending about a line beyond where the muscular and mucous tunics of the oviduct make the thin wall of the infundibulum opaque, its transparency against a dark ground contrasting with the opaque beginning of the proper tunics of the oviduct, which nevertheless are here very thin. No part of this delicate free margin is produced into fimbriæ; in this respect *Echidna* accords with *Ornithorhynchus,* and equally manifests the character by which the Monotremes differ from the Marsupials. The infundibular dilatation suddenly contracts about an inch from the opening into a "fallopian" tube, about a line in diameter, which is puckered up into four or five short close coils. The oviduct, after a slight contraction, suddenly expands into the uterus. This is about 2 inches long, and 6 lines in diameter. It commences by a short well-marked bend, convex outwards, and then proceeds nearly straight, the pair converging to the urogenital compartment, slightly contracting at its termination, which projects, as an "os tincæ," into the side of the fundus of that division of the cloaca.

The tunics of the uterus are, externally, the peritoneum, which is attached by a lax cellulosity to the "tunica propria;" this, with its fibrous or muscular layer, is thin, not exceeding $\frac{1}{8}$th of a line in the present specimen. The inner layer of the uterine wall is the thickest, and chiefly composes it, consisting of fine lamellæ stretched transversely between the fibrous layer and the fine smooth lining membrane, the whole being of a pulpy consistence, and doubtless in the recent animal highly vascular, especially in the impregnated state. The lining membrane was devoid of any trace of vascular connexion with the membranes of an ovum or fœtus, and was thrown into delicate irregular rugæ, which assumed the longitudinal direction at the "cervix" or contracted terminal part of the uterus. The orifice on the "os tincæ" was a puckered slit, about a line in extent; below it, on a produced or papillose part of the prominence, was the small circular orifice of the ureter.

The right ovarium was proportionally more developed and larger than

in the *Ornithorhynchus paradoxus*: three ovisacs were developed and attached, as in the left ovarium; and there was also a compressed ovisac, similar in size and shape to that in the left side, and exhibiting an apical cicatrix, whence it is to be inferred that, in this instance, the right as well as the left ovarium had furnished an impregnated ovum; and the near equality of size and close similarity of structure and condition of the right oviduct and uterus equally indicated that they had participated in the functions of the last season of generation.

The urinary bladder opened into the middle of the fundus of the urogenital compartment, the uterine orifices intervening between the vesical one and the ureters, as in the *Ornithorhynchus paradoxus*. The urogenital canal is 1 inch 4 lines in length, and about 9 lines in diameter; its inner surface shows by some coarse wavy longitudinal rugæ its capacity for dilatation. The rectum was here of great width; it terminated by a contracted puckered aperture in the back part of the beginning of the vestibule, behind the aperture of communication of the urogenital with the vestibular canal. The distal half of the vestibule is lined by a denser and less vascular epithelium than the proximal one. The author concludes, from these appearances, that the present *Echidna* had produced two young, of which only one was secured, and that probably she had a mammary fœtus in each pouch prior to her capture.

The one which was secured resembled the young of the *Ornithorhynchus* in the general shape and curvature of the body, and also resembled the new-born young of the Kangaroo in the proportions of the limbs to the body, in the inferior size of the hind pair, in the degree of development of the digits, especially of the fore pair, and in the feeble indication of eyes or eyelids. But the mouth is proportionally wider, and has the form of a transverse slit; it is not circular. Upon the upper lip, in the mid line between the two nostrils, is a small protuberance corresponding to that in the young of the *Ornithorhynchus paradoxus*, which had been covered by some epidermal production. The traces of ears are less conspicuous than in the young Kangaroo, the conch being little, if at all, developed in the mature Echidna.

The tail is much shorter than in the young Kangaroo, and shows as much proportional size as in the full-grown Echidna, in which it is a mere stump concealed by the quills and hair.

The head is proportionally longer and more slender in the marsupial fœtus of the Echidna than in that of the Ornithorhynchus or of the Kangaroo, and already at this early period foreshows the characteristic elongation and attenuation of that part in the mature animal. The form of the mouth, as a transverse slit, is a good monotrematous character of the young at that period, since, in all true or teated marsupials, the mouth of the mammary fœtus has a peculiar circular and tubular shape. A scarcely visible linear cicatrix at the middle of the lower part of the abdomen is the sole trace of umbilicus.

A bifid obtuse rudiment of penis or clitoris projects from the fore part of the single urogenital or cloacal aperture, and in advance of the base of the tail-stump.

The brain, of which the largest part was the mesencephalon, chiefly consisting of a vesicular condition of the optic lobes, had collapsed at this part, leaving a well-defined elliptical fossa of the integument, indicative of the widely open fontanelle at the upper part of the cranium *.

The skin of the shrunk body showed folds, indicative of the originally plump, well-filled abdomen.

The fore limbs, in their shortness and breadth, foreshowed the characteristics of those of the parent, which may be said, indeed, to retain in this respect the embryonic character, with superinduced breadth and strength. The digits have already something of the adult proportions, the first or innermost of the five being the shortest; the others of nearly equal length, but graduating shorter from the third to the fifth. The characteristic disposition of the digits was better marked in the hind limb, the second already being the strongest and longest, the rest more rapidly shortening to the fifth than in the fore leg. The innermost, agreeably with the law of closer retention of type in the embryo, though the shortest of the five, was less disproportionately so than in the adult.

After entering into other particulars, and quoting from correspondence on this subject of animal physiology, the author summarizes the chief points in the generative economy of the Monotremes, which still remain to be determined by actual observation as being—

1. The manner of copulation.
2. The season of copulation.
3. The period of gestation.
4. The nature and succession of the temporary structures for the nourishment and respiration of the fœtus prior to birth or exclusion.
5. The size, condition, and powers of the young at the time of birth or exclusion.
6. The period during which the young requires the lacteal nourishment.
7. The age at which the animal attains its full size.

In respect to the second point: as the female Echidna with the young, described in the present paper, was captured on the 12th of August, she might be impregnated at the latter end of June or in July. Females therefore, killed in the last week of July and the first week in August, in the province of Victoria, would be most likely to afford the capital facts noted under the "fourth" head, viz. the impregnated ovum *in utero*,

* In observations and illustrations of the development of the brain in Marsupialia (Phil. Trans. 1834), it is shown that, in the Kangaroo, some time after birth the chief and largest part of the brain is the mesencephalon, that the cerebellum is not more advanced than it is in Batrachian Reptiles, and that the cerebral hemispheres are even less developed: amongst the figures illustrative of this stage of formation of the marsupial brain is one of a dissection of the rudimental hemisphere, showing the large and simple ventricular cavity then occupying it.

showing some stage of embryonal development in the spring terrestrial Monotreme. As to the hairy and aquatic *Ornithorhynchus*, the impregnated females in which ova were found in the uterus, of small size, and prior to the formation of the embryo, were caught on the 6th and 7th of October. Young *Ornithorhynchi*, measuring in length in a straight line 1 inch and $\frac{7}{8}$ths, were found in the nest on the 8th of December. The period of impregnation, therefore, in this species, in the locality of the Murrumbidgee River, is probably the latter end of September or beginning of October. Females captured in the latter half of October and in the month of November, would be most likely to have ova *in utero*, exhibiting stages of embryonal development.

The author concludes by quoting a letter informing him that an Ornithorhynchus in captivity had laid two eggs, with a soft unvascular covering, each about the size of a Crow's egg. They were destroyed without examination. Had they been preserved in spirits or opened on the spot, the inference of the ovo-viviparous character of the animal might have been confirmed or otherwise. According to the Report, these alleged eggs must have resembled those of the Viper. Now the young Viper is provided with a specially and temporarily developed premaxillary tooth, for lacerating the soft, but tough, shell of its egg, and so liberating itself. From this analogy, the author conceives that the young Monotremes may be provided with a horny or epidermal process or spine upon the internarial tubercle for the same purpose. This temporary tubercle is obviously homologous with the hard knob on the upper mandible of chelonians and birds, by which they break their way through the harder calcareous covering of their externally hatched embryo.

The paper was illustrated by drawings of the female *Echidna*, of her marsupial pouches and young, of the mammary glands, and of the female organs of generation.

March 9, 1865.

Major-General SABINE, President, in the Chair.

The following communications were read :—

I. "Numerical Elements of Indian Meteorology.—Series II. Insolation, and its Connexion with Atmospheric Moisture." By HERMANN VON SCHLAGINTWEIT. Communicated by the President. Received December 27, 1864.

(Abstract.)

The author regards as an approximate measure of insolation the difference of the maximum temperatures observed by two similar thermometers,

one in the sun, and the other in the shade, disturbing influences being as much as possible avoided in both cases, and the observations being confined to those days on which the sun shone sufficiently clearly to cast a distinct shadow during some part of the interval between noon and 4 P.M. Comparing the differences of insolation in different parts of India and in different seasons, he is led to regard insolation as dependent greatly on relative humidity. Thus, generally speaking, it is greater on the seaboard than in the interior of India. At individual stations, the maxima of insolation occur on days of great relative moisture, *i. e.* on days in the rainy season when the clouds are temporarily broken, or in the months immediately following the rainy season, when the atmosphere is still very humid. Calcutta and Columbo are taken as types of a sea-climate, Konagheri and Bellori as types of an interior or very dry climate. In the one type the relative humidity is from 88 to 93, the insolation 50°; whilst in the other type the relative humidity is from 60 to 65, and the insolation from 8° to 11°. Still more striking results are obtained by comparing the mountain climates of Sikkim and Ladak, nearly at similar absolute altitudes. At Ladak the relative humidity is about 30, and the insolation about 18°; whilst in Sikkim the relative humidity is estimated at from 81 to 84, and the insolation from 60° to 75°. The contrasts in these comparisons are very great, and, with other examples, which are cited, appear to substantiate a connexion between the presence of aqueous vapour in its transparent state, and insolation as measured by the differences of thermometers in the sun and shade. The connexion is shown to be in perfect harmony with the results obtained by Professor Tyndall, and is explained by considering simultaneously the gain of heat which the thermometer experiences by direct radiation from the sun, and its loss of heat by radiation to the surrounding air. The opacity of the air for the invisible heat radiating from the thermometer rapidly increases with the amount of vapour of water which the air contains, while its transparency for the heat directly radiated from the sun is *comparatively* little affected. Thus when the air is highly charged with moisture, free radiation from the thermometer is much impeded; or rather, what the thermometer loses by radiation into the air is in some measure restored by radiation back again from the air.

II. "On the Structure and Development of the Skull of the Ostrich Tribe." By William Kitchen Parker, Esq. Communicated by Prof. T. H. Huxley. Received February 23, 1865.

(Abstract.)

The earliest condition of the struthious skull described by the author is that of a "pullus" of the African species, at about the end of the first third of the period of incubation. There are two individuals in this stage

from the stores of the Hunterian Museum *; and two others also, twice as perfect, from the same source; of these, one was more advanced than the other.

Next to these, in point of growth, is a young Freckled Emu (*Dromæus irroratus*, Scl.); this wanted one week of the full period.

The young of the Nandu (*Rhea*) were all ready for hatching, but died in the process: these chicks had the long-billed kind (*R. macrorhyncha*, Scl.) for their sire, but their dam was of the ordinary kind, viz. *R. americana*. Dr. Bennett's beautiful Cassowaries, the "Mooruks," have yielded two ripe pulli; these were both hatched alive at the Society's Gardens. The author has been able to give the condition of the Emu's skull at six weeks old, also at two months, at half a year, and likewise in the adult state.

The skull of the adult *Dinornis* is also described; and the so-called *Dinornis casuarinus* is shown to be a gigantic *Porphyrine* Rail.

The writer is indebted to M. Blanchard's work (L'Organisation du Règne Animal) for a knowledge of the condition of the skull of the immature *Apteryx*.

The Tinamous (*Tinamus robustus* and *T. variegatus*) were both old birds; but their skulls are rich in persistent sutures, and in bones which, although common amongst the cold-blooded classes, are rare enough amongst birds. Moreover naturalists have misplaced the Tinamous, by putting them with the "Gallinaceæ."

In this paper the bones formed in *membrane* merely, and those formed by the metamorphosis of true or hyaline *cartilage* are carefully distinguished; moreover the relation of the parts is displayed by sections made in various directions.

The figures are nearly all magnified, and they are coloured in a map-like manner, so as to display, by an exaggeration of the natural tints, the histological condition of the various parts of the skull and face at each stage.

The nomenclature of the parts is, on the whole, in harmony with that adopted by Professor Huxley in his recent work on Comparative Anatomy; but there are several new terms †, for which the author is responsible; they were imperatively called for, or they would not have been coined, and they are as much like the old human-anatomy names as possible. In this paper it is shown that the Ostriches are, on the whole, inferior to Birds generally, and yet that they come nearer to the Mammalia than the higher types; they are compared to the Cartilaginous *Fishes*, to the Amphibious *Reptiles*, and to the Marsupial and Monotrematous *Mammalia*.

After a minute description of the struthious type of skull, an "Ap-

* The young African ostriches were lent to me by the Council of the College, through the intercession of my kind friend Mr. W. H. Flower, the Conservator of the Museum. Most of my specimens of the other kinds I owe to Dr. Sclater and Mr. Bartlett; they came from the Gardens of the Zoological Society.—W. K. P.

† Most of these terms have already appeared in print in the author's paper on the "Gallinaceæ and Tinamous" (Trans. Zool. Soc. vol. v., 1864).

pendix" is given; and here the author takes occasion to describe much earlier stages of the skull in typical birds, viz. in the Crows. The primordial parts of the facial arches are carefully compared, beginning at the lowest Fishes, and ascending to the Mammalia; the pattern and *habit* of growth of the facial structures in the higher classes is shown to be adumbrated by the condition of these parts in the Lamprey (*Petromyzon*). The essential independence of the two arches in front of the mouth is asserted, and their low type of development is shown in the non-segmentation of the parts that should answer to the free *post*-stomal rays, the mandible, and the hyoid arch.

A survey is also made of the system of secondary bones—bones which have no preexistent hyaline cartilage as their basis; and these are shown to pass insensibly into dermal plates: the only distinction that can be made, viz. into *dermal, subcutaneous,* and *aponeurotic bones,* is there explained to be merely *useful,* but not to have anything embryologically essential in it.

March 16, 1865.

Major-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Dr. Watson proposed, and Dr. Sharpey seconded, the Right Honourable Lord Justice Turner for election and immediate ballot.

The ballot having been taken, Lord Justice Turner was declared duly elected a Fellow of the Society.

The following communication was read:—

"On the Magnetic Character of the Armour-Plated Ships of the Royal Navy, and on the effect on the Compass of particular arrangements of Iron in a Ship." By FREDERICK JOHN EVANS, Staff-Commander R.N., F.R.S., Superintendent of the Compass Department H.M. Navy, and ARCHIBALD SMITH, M.A., F.R.S., Corresponding Member of the Scientific Committee of the Imperial Russian Navy. Received March 9, 1865.

(Abstract.)

This paper contains a reduction and discussion of all the observations of deviation and of horizontal and vertical force made in the armour-plated ships of the Royal Navy, and also in certain iron-built ships of the Royal Navy and of the mercantile marine. It may be considered as a continuation of a paper on the Deviation of the Compass in iron-built ships of the Royal Navy, by Staff Commander Evans, published in the Phil. Trans. for 1860, p. 337.

The reduction gives the numerical values of the several parts of the

deviation, viz. the "constant," the "semicircular," and the "quadrantal" of λ, or the proportion of the mean force to north on board to the force to north on shore—of μ, the proportion of the vertical force on board to that on shore, of χ the heeling coefficient to windward; also of the several constituent parts of these coefficients.

The following are the principal conclusions derived from these values:

The introduction of armour plating, and the great increase in the amount and thickness of iron used in the construction of modern ships of war, have greatly increased the amount of the deviations previously considered, and have given importance to two sources of error not previously considered, viz. the diminution of the directive force, and the heeling error. To determine these, observations of horizontal and vertical force are necessary, and they are now part of the regular series of observations made by the Superintendent of the Compass Department in the iron-built ships of the Royal Navy.

For the formulæ by which the reductions are made, and which are derived from Poisson's General Equations, reference is made to the 'Admiralty Manual for ascertaining and applying the deviations of the compass caused by the iron in a ship,' 2nd edition, London, 1863, edited by the authors. This work has been translated into French by M. Darondeau, into Russian by Captain Belavenetz, of the Russian Navy, and into German by Dr. Schaub, the Director of the Hydrographical Department of the Austrian Navy.

The observations confirm the conclusion formerly obtained, that the semicircular deviation in an iron-built ship is chiefly due to the attraction of the north point of the compass to the part of the ship which was *south* in building, modified in armour-plated ships by the direction of the ship while being plated.

The observations also show the rapid changes which take place in the semicircular deviation soon after launching, and the considerable changes which take place for about a year afterwards, and the great permanence of the semicircular deviation after that time.

Observations are yet wanting from which the separation of the principal part of the semicircular deviation B into its two constituents can be derived with much certainty. The following can only be looked on as approximate.

	B in England.	Part of B from soft iron.	Part of B from hard iron.
Warrior	$-24\frac{1}{4}^{\circ}$	$+12^{\circ}$	$-36\frac{1}{4}^{\circ}$
Black Prince	$+23$	$+23$	0
Defence	$+25\frac{3}{4}$	$+14\frac{1}{4}$	$+11\frac{1}{2}$

The great difference in the values of the last part in the 'Warrior' and 'Black Prince' depend on this, that the 'Warrior' was built head north, the 'Black Prince' head south.

In the iron-built armour-plated ships the quadrantal deviation becomes very large, very much exceeding what has been found in other ships. This, however, is not to be attributed in all cases to the armour-plating, as is shown by the small values of the quadrantal deviation in the wood-built armour-plated ships; and theory as well as observation shows that, in the case of a compass in a central position, the armour-plating rather tends to diminish the quadrantal deviation. The different amount in the different ships is completely accounted for by the position of the bulkheads and armour-plating, rifle-towers, &c.

Thus in the following cases, in which the position of the armour-plating relatively to the compass is such as to increase the quadrantal deviation, the values are,—

	Warrior.	Black Prince.	Achilles.	Defence.	Resistance.
Standard compass	8° 27′	7° 38′	° ′	7° 0′	6° 17′
Steering compass	11 56	10 32	....	10 16	8 28
Main-deck compass	11 43	13 6	12 13	14 35	14 0

In the following cases of iron-built ships, the armour-plating is so placed as to have little effect on the quadrantal deviation.

	Achilles.	Hector.	Valiant.
Standard compass	6° 58′	5° 24′	4° 54′
Steering compass	8 51	8 24	6 52
Main-deck compass	....	9 47	8 05

In the following wood-built armour-plated ships, the armour-plating being carried all round, and the compasses near the centre of the vessel, the effect is to diminish the quadrantal deviation.

	Royal Oak.	Prince Consort.	Caledonia.	Ocean.
Standard compass	3° 09′	2° 18′	2° 57′	2° 31′
Steering compass	1 47	....	....	....
Main-deck compass.....	1 28	....	....	....

In one of the turret ships, where the compass was out of the midship line, E, the other part of the quadrantal deviation, attained a large amount, being 4° for one compass, and 9° for the other. In all the cases of compasses in the midship line, E is small.

The diminution of the directive force in these ships is also remarkable. In the main-deck compasses of some of the iron-built armour-plated ships the mean directive force hardly exceeds $\frac{7}{10}$ of that on shore.

The most remarkable feature, both in the quadrantal deviation and in

the diminution of the directive force, is the constant and regular diminution of both effects with the lapse of time, showing apparently a change in the molecular structure of the iron, by which it becomes less susceptible of induced magnetism. This change has not yet been connected with any observations on the strength of the iron.

The amount of heeling error in these ships is very considerable, averaging about 1° for every degree of heel. In those which have been built head north it is greater. Thus in the 'Warrior,' which was built head north, it is at the standard compass aft 1° 49′ for every degree of heel. This error may be corrected by means of a vertical magnet. In the wood-built armour-plated ships, from the armour-plating causing the vertical force to act upwards, the heeling error is very small, and generally to leeward. Thus it is, for each degree of heel for the standard compass, in the following ships—

Royal Oak............	7′ to windward.
Prince Consort	8′ to leeward.
Ocean	15′ to leeward.

The method of obtaining the heeling error by observations of horizontal and vertical force, in addition to observations of deviation, is practically used in this paper for the first time. The formulæ for the purpose were given for the first time in the 'Admiralty Manual.'

Among the practical conclusions drawn by the authors, the most important are, that the best position for the ship to be built in is head south; that armour-plated ships should be plated with the head in the opposite direction to that of building; that there should be as little iron as possible within a cone traced out by a line passing through the compass, and making an angle of 54° 45′ $\left(\cos^{-1}\frac{1}{\sqrt{3}}\right)$ with the vertical; and that in the construction of iron-built and iron-plated ships, regard should be had to providing a suitable place for the standard compass.

The separation of the constituent parts of the various coefficients is not only of great scientific interest, as giving the causes of the different amounts of these parts in different ships, but some of these quantities are so nearly the same in ships of the same class, that when a sufficient number have been observed, we are able, by means of observations of deviation and horizontal and vertical force, made without swinging a ship, and even when she is on the stocks or in dock, to construct by anticipation a table of deviations and of the heeling error. This method was applied in certain of the cases given in the Tables when there was not an opportunity of swinging the ship. This method may be expected to be of much use when the selection of a place for the standard compass comes to be considered part of the duty of the naval constructor.

The second part of the paper treats of the effect on the compass of masses of iron of various shapes, bearing some analogy to shapes for

which the problem of the distribution of induced magnetism can be exactly solved. It is known that when a uniform mass of iron is magnetized by induction in a uniform field of force, the effect of the whole magnetism induced throughout the mass is precisely the same as that of a certain distribution of free magnetism on the surface (including, in the case of a hollow shell, a distribution on the inner surface), the amount and law of this distribution depending on a coefficient κ, which is zero for non-magnetic bodies, and infinite for a body infinitely susceptible of induction. Very few observations of the value of this coefficient have been made. The only observations of which the authors are aware, made for this special purpose, are those by Weber (Götting. Trans. vol. vi. p. 20), Thalen (Nov. Act. Soc. Reg. Upsal. 1861), and by the authors. Weber finds for hard steel $\kappa=4{\cdot}934$, for soft steel $\kappa=5{\cdot}61$, for soft iron $\kappa=36$. Thalen finds for soft iron κ varying from 27·24 to 44·23, the mean being 36·75. The authors find, for a rod of iron probably not very different from the iron used in the construction of iron ships, $\kappa=12$ when the iron is not struck between reversals, but when hammered sharply it rose to upwards of 80. The effect of rods or plates magnetized longitudinally is nearly proportional to κ; but when a mass is magnetized at right angles to its surface the case is very different, and the free magnetism is almost independent of κ. Thus in the case of a plate magnetized at right angles to its surface, in the case of a sphere, and in the case of a cylinder magnetized at right angles to its axis, the free magnetism is proportional to

$$\frac{4\pi\kappa}{1+4\pi\kappa}, \quad \frac{\frac{4}{3}\pi\kappa}{1+\frac{4}{3}\pi\kappa} \quad \text{and} \quad \frac{2\pi\kappa}{1+2\pi\kappa} \text{ respectively,}$$

which are so nearly independent of the value of κ, that the effect of a sphere of the hardest steel magnetized by induction is within 4 per cent. of the effect of a similar sphere of the softest iron, and the effect of the latter within 1 per cent. of what it would be if the iron were infinitely susceptible of induction. Hence the magnetism of thin masses of iron depends very much on the quality, and also on whether the iron is hammered or not. The magnetism of thick masses of iron is almost wholly independent of these circumstances.

One of the most interesting applications of the formulæ is the comparative effects of solid and hollow spheres, and bodies of analogous shapes.

The proportion of the effect of a solid sphere to that of a spherical shell of thickness t (in terms of the radius of the sphere) is as $t+\dfrac{3}{8\pi\kappa} : t$.

In the case of soft iron this is about

$$t+\frac{1}{300} : t,$$

so that when the thickness of the iron considerably exceeds $\frac{1}{300}$ of the radius of the sphere, the effect of the spherical shell is sensibly the same as that of a solid sphere of the same external diameter. Mr. Barlow found

that when $t=\frac{1}{150}$, the above proportion was as $3:2$, which gives for κ a value $=35$, agreeing closely with the values found by Weber and Thalen. In the case of iron of the quality experimented on by the authors, this ratio would be

$$t+\frac{1}{100}:t.$$

Hence in the case of a tank $\frac{1}{10}$th of an inch thick and 4 feet in diameter, t would be about $\frac{1}{250}$, and the effect about $\frac{1}{3\cdot 5}$ that of a solid mass of iron of the same size. These results, which, however, are not new, as they are involved in Poisson's paper of 1824, explain the mistakes into which various magneticians have fallen as to the magnetism residing entirely on the surface, and as to the effect of a body such as a tank depending on its surface, not on its mass.

The same formulæ show that to correct a quadrantal deviation of 6° 10′ by two cannon-balls placed one on each side of the compass, the distance of the centre of each ball from the centre of the compass should be three radii of the balls. If the distance is greater or less, the quadrantal deviation corrected will vary inversely as the cube of the distance.

The investigation also shows that the effect of a sphere of iron, if its centre be within the cone of 54° 45′, will be prejudicial both by diminishing the directive force, and by increasing the heeling error to windward. When without that cone it will be beneficial in both respects. Hence, as far as possible, no iron should be within this cone.

Masses of iron which may be compared to a sphere, and near the level of the compass, but in the fore or aft quadrant, are beneficial in increasing the directive force, but prejudicial in increasing the quadrantal deviation. If they are on the port or starboard quadrant, they are doubly beneficial, by increasing the directive force and diminishing the deviation.

Bodies which may be compared to infinite vertical cylinders, such as iron masts placed before or abaft the compass, are prejudicial by increasing the quadrantal deviation, and they do not increase the directive force.

March 23, 1865.

Major-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Count Strzelecki proposed, and the Master of the Mint seconded, the Right Honourable the Earl of Donoughmore for election and immediate ballot.

The ballot having been taken, the Earl of Donoughmore was declared duly elected.

The following communications were read:—

I. "Inferences and Suggestions in Cosmical and Geological Philosophy." By E. W. Brayley, F.R.S. Received February 23, 1865.

Theory of the Sun—Synthesis of Ponderable Matter in the Sun—Cause of the Solar Spots—Production of the Zodiacal Light—Origin of Meteorites—Original Formation of the Planets—Discrimination of the Views in Cosmical Philosophy advanced from those of Mayer and his School—Theory of the Minor Planets—Projectile Power of the Sun.

(Abstract.)

This paper commences with the "*Theory of the Sun,*" embracing the subjects of the source of its energies, and the synthesis of ponderable matter. The position, powers, and functions of the Sun, as the physical centre of the solar system, are peculiar, and in fact unique. The "Primary Induction" from them, indicating, in the author's opinion, "the principle of philosophical investigation" which should be applied to the Sun, is conceived to be "That they imply a corresponding uniqueness and peculiarity in its constitution, characterizing also the nature as well as the disposition of the substances of which it essentially consists. But the particular density of the Sun indicates that it actually consists both of ponderable and imponderable matter. The nature of the former as constituting apparently its relatively exterior regions [is] believed to be made known in part by Professor Kirchhoff's researches in Prismatic Chemistry applied to the Sun, as showing that some of the elementary substances of the Earth exist also in the Sun"*.

The first obvious verification of this primary induction is presented by the "form of the Sun, which according to the observation of the equality of its diameters, is that of a perfect sphere, a form which is unique in the solar system, and is probably unknown in Terrestrial Nature"*.

The cardinal peculiarity of the Sun, that in which it is unique in the highest sense, is that its radiation exclusively possesses the property of imparting to (inorganic) matter a fit condition for the manifestation of (organic) life; that it is, humanly speaking, infinite in amount, and also the source of all the heat and light, and consequently of all their derivative or correlate forces which are active in the solar system. The illustration of the temperature and expenditure of heat of the Sun founded by Sir John F. W. Herschel upon his own experiments and those of M. Pouillet, with Mr. Waterston's experimental result that the potential temperature of an infinitesimal area of the Sun's radiating surface is nearly thirteen millions of degrees of Fahrenheit's thermometer, are cited as being in fact nothing more than philosophical confessions that no proportion whatever can be established between any expression for the solar energies and the obvious reality of their incalculable amount.

The author proceeds to inquire what we may reasonably conceive to be

* Syllabus of Lectures on Astronomical Physics, delivered at the London Institution in 1864, here cited from a revised edition printed for private use. Lecture V.

the intimate nature of matter in its highest and most elementary character, such as that essential to the Sun is in this paper inferred to be, agreeably to the principle of philosophical investigation he has suggested as being alone applicable to the Sun, and according to known facts and recognized principles of science. The answer to this question is afforded, he conceives, by modern views of the constitution of gaseous substances as forms of ponderable matter, and of that of the luminiferous ether as an imponderable body. It is deduced from the former, combined with the investigations of philosophers during the last half century, including those of the late Dr. Thomas Young and M. Cauchy, and of M. Neumann and Professor Stokes, that the ether is characterized by enormous molecular activity, rendering it immensely rarer but at the same time more truly solid and elastic than any kind of ordinary or ponderable matter, all forms of which it pervades, even the most dense, coexisting with them in the same space.

The author infers that the substances peculiar to the Sun transcend the ether in these qualities in even a greater degree than that excels ponderable matter with respect to them—that is, succinctly, that pure solar matter is still more transcendently and intensely solid and elastic because its particles are in still more transcendent activity. This inference is considered to harmonize with the obvious peculiarity of the Sun, that in it alone, of all the bodies and localities of the solar system, enormous force of gravity and an immeasurable intensity of heat are united.

In the recorded facts of telescopic observation, the author finds an "entire absence of evidence or indication that anything exists in the sensible universe which is of greater antiquity than the stars, or prior to them in its origin;" which, "considered together with the primary induction from the uniqueness and peculiarity of their position and functions as suns," is regarded as tending strongly "to prove that, as a class, the stars are the most ancient objects in the Creation, and also (each in its own sphere of action) the origins of the series of physical agencies and processes by which the planets and other classes of heavenly bodies were finally produced and are maintained"*.

This being admitted, it follows that the original production of ponderable matter takes place in the stars, and in our Sun as one of them,—a conception to which the author had been led by the preceding and other considerations long before the application of prismatic chemistry to the Sun.

The energy set free in the *condensation* within the Sun, of the highest imponderable matter essential to it into ponderable matter (an expression which is shown not to be a solecism), and eventually into the metallic vapours which the observations of Kirchhoff and other spectroscopists have discovered in the Sun and other stars, is inferred by the author to be, at once, the exclusive proximate source of the heat and light and other energies of the Sun, and (in our solar system) the only and universal

* Syllabus of Lectures on Astronomical Physics. Lecture VIII.

origin of ponderable or ordinary matter, the absolute synthesis of which from its imponderable elements is thus believed to take place in the Sun.

It will follow that the distribution of heat in the Sun, as already inferred by the author*, is from within to without, in the order of decreasing intensity—an inference which he conceives to be not contradicted, but supported, by the apparently inferior calorific and luminous condition of the Sun's nucleus, as disclosed in the true nuclei or inner umbræ of the spots, compared to that of every other visible part of the Sun, and especially of the photosphere, because the exterior regions of the Sun are composed entirely of that order of matter—ponderable matter, such as that of which the planets consist—which is alone capable of communicating to the ether of space those vibrations which we know as heat and light.

The "*Cause and Nature of the Phenomena termed the Solar Spots*" are next considered. The energy arising from the transition of imponderable into ponderable matter, will in part become the centrifugal or projectile force by which the torrents of matter (finally assuming the gaseous form) so produced are impelled through the denser envelopes of the Sun, causing the spots and the other phenomena of ebullition of which the photosphere is the scene.

The rotation of the Sun acting upon these torrents issuing radially from its interior regions—and probably from the surface of the solar globe disclosed in the true nuclei of the spots, or somewhat within that—by inflecting them towards the Sun's equator as they rise, occasions the actual distribution of the spots which are their outbursts on the surface of the photosphere, in lines and belts parallel to the equator, their restriction in latitude to within a certain distance of it, and their absence, together with that of the *faculæ,* both in the equatorial and the polar regions of the Sun. It would seem that the originally uniform evolution of aëriform matter from the entire surface of the interior globe, on being swept towards the equator from each pole, is broken up into torrents by the same general cause, and by the resulting inequalities of temperature, density, and consequent resistance in the surrounding and incumbent mediums. The region of equatorial calm, or freedom from spots and faculæ, is the result of the meeting and mutual opposition of the systems of currents which the Sun's rotation causes to proceed from either pole, the torrents being carried back in each hemisphere by movements of the nature of circular waves of translation seemingly affecting all the envelopes of the Sun, and setting from the plane of junction at the equator towards the tropics.

The entire assemblage of actions now under consideration appears to be closely analogous to that exhibited by a liquid boiling violently and incessantly from a heated surface below, the gaseous matter evolved at which becomes partly diffused through the liquid by adhesion or mixture, is partly disseminated through it in bubbles which collapse at various depths, and partly escapes by effervescence at its upper surface. In the actual case of

* Companion to the Almanac for 1864, p. 51.

the Sun, torrents more or less permanent, consisting either of bubbles or of an unbroken stream of aëriform matter, are also formed by the operation of the controlling mechanical cause to which, acting in the second place, the entire system of phenomena presented by the spots is here attributed. These torrents are continuously maintained from near the surface of origin on the glowing sphere within to that of the photosphere without, whether in single or in groups of separate but probably often confluent streams, elevating the photosphere into faculæ by the force of expansion with which they burst upon it, and, being transparent, permitting the interior envelopes and the incandescent nucleus to be seen through them.

The system of currents necessarily produced by the heat-action in the inferior liquid mediums and photosphere, combined with those due to the sun's rotation, carry along with them the torrents and their bursting summits in the drifting motion observed by Mr. Carrington to affect the spots. The phenomena of the spots generally, especially as described by that astronomer, are in entire conformity with this interpretation of them. Their control by the Sun's rotation was first perceived and announced by Sir John Herschel, in connexion, however, with his cyclonic theory of their nature.

The observed spherical form of the Sun is considered to be preserved by the perpetual escape from its equatorial regions, by means of the ebullition of the spots, of matter which in consequence of the Sun's rotation would otherwise accumulate upon them and so cause a deviation from the spherical form. Being thus separated, it receives from the Sun's rotary motion at the equator the form of the *Zodiacal Light*, which it thus constitutes, while the perpetual supply of fresh matter from the solar surface causes it to be, not a ring, but a lenticular mass, geometrically though not physically continuous with the originating central body, which it thus envelopes, no interval apparently being left between them.

The next objects of discussion are the "*Origin of Meteorites, Series of Physical Processes of which they are the result, and their Functions in Nature.*"

The vapours of metallic and other elementary matter evolved or discharged in the ebullition of the photosphere above considered, partly remain upon the Sun, constituting its atmospheres*, but are principally aggregated into masses of immense magnitude (terrestrially speaking) of the nature of bubbles, which, having undergone a certain amount of condensation, first become visible as those particles the collective brightness of which reveals to us the existence of the zodiacal light, being, in fact, the matter separated from the Sun's equator as explained above. These particles, termed by the author *meteoritic masses*, are projected from the zodiacal light by the force to which its variable extension is owing, and are further gradually condensed during their passage through the interplanetary spaces into the liquid and solid state, constituting eventually the nuclei of Meteors, which are finally precipitated upon the Earth (and doubtless upon the other planets) in the form of METEORITES.

* Companion to the Almanac for 1864, p. 46; for 1865, p. 53.

The sudden outburst of light over a solar spot witnessed on September 1, 1859, by Mr. Carrington and Mr. Hodgson, the author regards as a fact confirmatory of these views, and as having been the consequence or accompaniment of the production, and the transfer with immense rapidity from within to without some exterior region of the Sun, of a meteoritic mass, or more probably of an immense congeries of such masses, enabled, by its consisting of ponderable matter, to manifest the higher temperature and consequent greater effulgence of the interior regions of the luminary, whence it was originally derived. Certain phenomena before recorded by astronomers but not yet understood are probably of the same nature.

The structural characters of meteorites are those of bodies which have been originally condensed from heterogeneous vapours—the mingled vapours of uncombined elementary substances variable in their nature and requiring different temperatures for their maintenance in the gaseous form, but all existing originally at a very high temperature; and their adequate investigation may afford, as an *experimentum crucis*, an independent confirmation of Kirchhoff's discovery, and of the truth of the spectrum-analysis of the composition of bodies distant from us in space. They consist, mineralogically, of two groups, meteoric iron and meteoric stones, forming, however, by graduation into each other, as first pointed out by the author, many years since, one series of bodies*. The intermediate examples, and indeed most of the stones, are aggregates of earthy matter partly in the crystalline and partly (as Mr. H. C. Sorby has shown†) in the vitreous state, and distinct portions of metallic iron alloyed with other metals. They are, in fact, always heterogeneous aggregates, in conformity with the origin here assigned to them. While as a class meteorites are perfectly distinct from all terrestrial rocks—the presence of metallic iron as a mineral constituent imparting to them indeed a character which is perfectly unique—some of their constituent minerals, and all the elementary substances of which they are composed are such as are found, but differently associated, in the Earth's crust, although there are many other terrestrial elements which have not yet been discovered in them.

"Ten, or perhaps more, of the elements of the solar atmosphere," according to Kirchhoff and Ångström, "are also those of meteorites—iron, nickel, cobalt, chromium, and magnesium being characteristically such. But the non-metallic base silicon, which, in union with oxygen as silica, is an abundant and equally characteristic element of meteorites, is absent in the Sun, according to our present knowledge, in which also other elements of meteorites, including oxygen itself, are not known to be present"‡. It cannot be doubted, however, that by the further prosecution of spectrum-analysis other elements will be discovered in the Sun. It must be remembered also that our knowledge of meteorites is confined to a few only of

* Annals of Philosophy (January 1824), second series, vol. vii. p. 73; Philosophical Magazine (December 1841), third series, vol. xix. p. 501.

† Proceedings, vol. xiii. p. 333. ‡ Companion to the Almanac for 1865, p. 65.

those which have fallen upon the Earth, and that during a very small space of time, physically speaking, not exceeding a few thousand years, or perhaps even not many centuries; while the synthesis of ponderable matter in the Sun may reasonably be supposed to vary from time to time as to the particular chemical elements produced. A remarkable and instructive fact, in the actual condition of science on this subject, is that the metal iron is now known to be an abundant and characteristic element of the Sun, of Meteorites, and of the Earth.

In harmony with these views on the origin of Meteorites is a recorded but perhaps hitherto unpublished opinion of Sir H. Davy, that they originally consist of the metallic and other combustible bases of the earths and alkalies of which Meteoric Stones are principally composed. But whether the oxidation of these bases is effected in the Earth's atmosphere, as he also suggested, or whether in some cases, though not in all, oxygen is present in the original assemblage of elementary vapours, and combines with certain bases and with portions of others, as the condensation proceeds, is a difficult question. The latter theory may be thought to agree better with the entire series of phenomena presented by Meteors, and with the constitution of Meteorites as a peculiar class of mineral aggregates; but some facts relating to either branch of the subject tend to support the former. Both may be true to a certain extent. The facts, however, that scarcely any oxidation of the iron Meteorites has taken place, and that there are no meteorites which consist principally of oxide of iron, while there are some in which metallic iron and earthy matter (oxides) are present in nearly equal proportions, but that even in these no excess of oxide of iron occurs, are opposed to the supposition that meteorites have derived any considerable part of their oxygen from the atmosphere; with which also the existence of sulphide of calcium in certain meteorites is inconsistent.

In what part of Space between the Zodiacal Light and the Earth the final condensation takes place is not at present determinable. It would seem that these masses must retain much of their original heat and therefore to a great extent an aëriform or vaporous condition (though one of greater density than that in which they left the Sun or even the Zodiacal Light, and mingled with liquid or solid matter as just suggested) in the interplanetary spaces where the ether alone exists, and that their entire conversion into a liquid and finally a solid form may not occur until their arrival in a region of positive cold in the vicinity of the Earth or other planets. Mr. Sorby has lately inferred from the equable manner in which mineral ingredients greatly differing in specific gravity as well as fusibility are mingled in meteorites, that their formation must have taken place in some physical locality where the force of gravitation is small; "that they come either from the outside of a very small planet, much less than the moon, or else from the *interior* of a larger planet, since broken up" *. The first inference is in perfect accordance with the theory of Meteorites announced in this paper; for it is evident that the force of gravity in the original meteoritic masses

* Letter to the Author.

must be very small, quite inadequate to interfere with the disposition within them, and among one another, of their proximate elements, however discordant in fusibility or specific gravity. It will follow, also, that the final condensation of these vaporous masses cannot take place either very near the Sun or very near the Earth.

According to observations of the author already published*, the iron meteorites, if not certain single Meteoric Stones (and most probably also the entire nucleus, which in some cases is broken up and falls as a shower of Meteorites), have the form (resembling that of the Meteors themselves, which is nearly that of a flame) of the solid of least resistance, or of one derived from it, and received in fact from the resistance of a medium they have traversed, but having in general one termination, and sometimes the other also, truncated to a variable extent. This would seem to prove that they must once have been—as individual masses, and not merely as portions of a body of which they originally formed part, nor as to their pre-existing materials only—in a fluid or mobile condition. These and other significant circumstances are adduced in the paper as tending to the discrimination of the physical changes by which meteoritic masses are affected prior to their entering the Earth's atmosphere, from those which they afterwards undergo within it and from its action,—the conclusion arrived at being that the solid Meteorite is finally left, with a slight alteration in figure, and however greatly reduced in volume, in the approximate actual form—that of a *bubble* elongated by its being impelled in a certain direction through a resisting medium—in which, when in a gaseous state, it left the Sun.

The phenomena of Luminous Meteors (Shooting-Stars and Fire-balls) more or less examined by physicists from the latter part of the preceding century (the author having himself endeavoured to elucidate certain characteristic phenomena of Fire-Balls by applying to them the results of modern science†), but which, since the appearance of the persistent Meteor-shower in November 1833, have been so assiduously observed and discussed by meteorologists, especially in relation to the periodicity they exhibit, are shown to be entirely conformable to the views of their origin which are enunciated in this paper. The petrological characters of Meteorites themselves, as recently investigated by mineralogists‡, together with others before noticed by the author§, are also accounted for by

* First announced in Lectures on Igneous Meteors and Meteorites given at the Royal Institution in 1839, and at the London Institution in 1841. See English Cyclopædia, Div. Arts and Sciences, "Meteors, Igneous or Luminous," vol. v. col. 604.

† See "A Sketch of the Progress of Science respecting Igneous Meteors and Meteorites during the year 1823," read before "the Meteorological Society" May 12, 1824, and published in the Philosophical Magazine (for October of the latter year), first series, vol. lxiv. pp. 288–292; also Second Supplement to the Penny Cyclopædia, "Meteors, Igneous or Luminous," and English Cyclopædia, as referred to in the preceding note.

‡ Reichenbach, Haidinger, G. Rose, Maskelyne, Sorby, R. P. Greg.

§ Syllabus of Lectures, on Igneous Meteors and Meteorites, delivered at the London Institution in 1841, as reprinted in Phil. Mag., third series, vol. xix. p. 501, with addition, p. 502.

these views, though, with respect to the former, in a very different manner from that hitherto accepted.

The long-continued study of Meteorites and of the phenomena which attend their fall, affected by the consideration of the probable synthesis of ponderable matter in the Sun, and—since the conclusions of Kirchhoff have been announced—the special study of Solar Physics and Chemistry in connexion with both subjects, appear to the author to justify him in entertaining the hope that he may thus have succeeded—by means, partly, of a new deductive cosmical hypothesis submitted for verification, and partly by uniting, and in some cases newly interpreting, preceding inductions on particular points of their physical history—in effecting at least the approximate solution of the problem of the Origin and Formation of Meteorites, which has been sought by philosophers from the time of the communication to the Royal Society, now sixty-three years since, of Edward Howard's paper, demonstrating their peculiar nature and establishing the reality of their fall *.

The succeeding section of the paper is headed *"Original Formation of the Planets: Origin of the Primitive Heat of the Earth, causes of its Permanence and Invariability; the Earth not a cooling body."* In this it is represented that the results of modern science conspire to prove that we must look to causes now in operation as those which have produced the planets. If—as first evinced by Mr. G. Poulett Scrope, with respect chiefly to volcanic and plutonic action, and secondly, but from a wider induction by Sir C. Lyell—they are sufficient to account for the phenomena of its surface and crust, as made known by Geology, it follows, by parity of reasoning, that they will be sufficient to account also for its original production. The only known phenomenon in which the process of the formation of the Earth as a planet is actually observed, is that of the fall of Meteorites upon it, by which its magnitude is augmented, and that by the addition of materials homogeneous with those of its existing elementary constitution, being chiefly those chemical elements which are present in the greatest quantity in the Earth's crust, and seem to be most essential to its constitution. The characteristic presence of iron in both has been already adverted to. According to the principle of the adequacy of Existing Causes, therefore, we must conclude that the fall of Meteorites is a continuation or a residue of the process of formation of our planet, and that the Earth was originally produced by the aggregation and coalescence of Meteorites, or of greater masses into which they had previously coalesced.

Agreeably to the law of the Conservation of Energy and to the dynamical theory of heat, the enormous original velocity of the Meteorites being diminished by their collision and coalescence, great part of the mechanical force of their motion would be reconverted into heat, and become eventually the "primitive" internal heat of the Earth, for which it would appear that what may be reasonably characterized as a *vera causa* is thus supplied.

* Read February 25, 1802; published in the 'Philosophical Transactions' for that year, Part I.

It is next shown in the paper in what manner a nucleus of hypogene rocks or plutonic granite supplying the materials for the subsequent deposition of sedimentary strata, and also the chemical elements of organic beings, would be one of the final results of the Earth's formation by the coalescence of meteoritic masses. The chemical action of its primitive and central heat, governed by the solar radiation upon the exterior of the new planet, would initiate the cycle of correlate activities by which its permanence would be secured. In addition to thus assigning a natural and adequate cause for the secular invariability of the Earth's internal heat, sometimes assumed as an axiom in geological speculations without being accounted for, these suggestions may evince that, on the other hand, it is unnecessary to regard the Earth as a cooling body.

Admitting the Earth to have been formed as here suggested, such also must have been the process of formation of the other Planets.

A "*Discrimination of these views in Cosmical Philosophy from those of Mayer and his School*" is here interposed. The induction by which the original formation of the Earth and other Planets is arrived at in this paper is new; but from that point these views have a certain parallelism with those founded on the "Celestial Dynamics" first enunciated by Mayer, for a knowledge of which English scientific literature is chiefly indebted to the zeal of Professor Tyndall. Of the Mayerian Theory that presented in these "Inferences and Suggestions" is virtually in nearly every stage the inversion, though not suggested by nor produced by inverting it, having been founded on different data and arrived at by independent reasoning. According to the physicists of the Mayerian school, the activities of nature begin with the mutual attraction of "Cosmical Masses" of which Meteorites are taken as examples*. In the theory now offered they commence with Force and Heat and Light and Matter locally originating in the Sun.

The "*Theory of the Minor Planets*" is next briefly considered. All the phenomena they present are regarded as supporting the conclusion that their peculiar relations and community of character are not, as hitherto supposed, effects of their having formerly constituted one heavenly body which has been reduced to fragments, but of their being bodies intrinsically of the same nature, meteoritic masses in fact, in an advanced intermediate state between the condition of meteorites and that of true planets, in process of gradual convergence towards each other, preparatory to their coalescence into one greater planet.

The last section relates to the "*Projectile Power of the Sun,*" accounted for in the section on the Spots, and by which meteoritic masses are conceived to be transferred with great velocity to the interplanetary spaces.

Everything here ascribed to the Sun is of course intended to apply in a general manner to the Stars also, so far as our knowledge of them extends; agreeably to the primary cosmical truth that they are Suns, "which must

* Companion to the Almanac for 1865, pp. 41–70.

be characterized, each in its own System, by the Uniqueness and Peculiarity which characterize our Sun in its System" *.

No suggestion is offered in this paper as to the remote origin of the solar elements, or that of the force by which they are conceived to be condensed into ponderable matter.

II. "On Zoological Names of Characteristic Parts and Homological Interpretations of their Modifications and Beginnings, especially in reference to Connecting Fibres of the Brain." By Prof. OWEN, F.R.S. Received March 10, 1865.

In a paper "On the Commissures of the Cerebral Hemispheres of the Marsupialia," &c., of which an 'Abstract' appears in the last published Part of the Proceedings of the Royal Society (No. 72), the author quotes the definitions of those structures given as zoological characters by me in a brief summary of the primary divisions of the class *Mammalia*, communicated to the Linnean Society in 1857.

The remarks on the signification and homology of those structures in my anatomical publications are not given, I am consequently misrepresented. Errors are imputed to me which the author deems it important to rectify before the Royal Society; and as the Proceedings of the Society will carry this imputation far and wide through the world of science, I venture to hope that the present defence will not be deemed uncalled for, but may be permitted to have place in the Serial which has diffused the attack.

In this I am moved, less on personal grounds, than in the interest of science and of scientific ethics; for of late a practice has arisen of representing a zoological definition of a part which an anatomist may have given in a classificatory work, as the exponent of his homological knowledge and descriptions of such part, in its various modifications and grades of development. Cuvier, for example, in his characters of the order *Bimana*, affirms that Man is the only animal possessing 'hands' and 'feet':—"L'homme est le seul animal vraiment *bimane* et *bipède*" †.

The *Quadrumana* are differentiated as having 'hands' instead of 'feet,' a 'hand' being defined as having the thumb opposable:—"Le pouce libre et opposable aux autres doigts, qui sont longs et flexibles" ‡.

The aim of the author in the zoological work above cited was to impart obvious and easily apprehended differential characters of the organ which observation had shown to define the groups.

The naturalist, thus enabled to place his subject in its proper class or order, is not concerned, as such, in knowing the homological or transcendental relations of the part or character which has afforded him the means of effecting what he wished to do.

* Syllabus of Lectures on Astronomical Physics, Lect. VII.

† Règne Animal, tom. i. p. 70, 1829.

‡ Ibid. p. 85.

Linnæus, to whom mainly is due the discernment of the powerful instrument of well-defined terms in acquiring a systematic Science of Nature, and to whom we owe our best knowledge of its use, so named the guiding parts of plants and animals, for such arbitrary or special application, in Botany and Zoology: for example, the 'bract,' the 'spath,' the 'sepal,' the 'petal,' are differentiated from the 'leaf,' as things distinct.

What would be thought of the botanical critic who, quoting the definition of the flowers of Cyperaceous plants, as consisting, for example, of 'glumes,' should meet the statement by a flat contradiction, as, viz., that they were nothing but little bracts,' and who, then, with a show of profounder research should proceed to expound the 'bract' as being the first step by which the common leaf is changed into a floral organ? The answer is obvious. But what next might be said, if it were pointed out that the objector had obtained this very notion from the 'Prolepsis Plantarum,' or other homological writings of the author criticised, where such philosophical considerations, foreign to the classificatory work, were the proper aim and object? So, with regard to the zoological definitions and characters of Cuvier. Those which I have cited might be met by as flat contradictions: such as that, "The 'hind hands' of the Quadrumana are nothing but 'feet'"; and the contradictor might then proceed to demonstrate, with much show of original research, the homology of the 'astragalus,' 'calcaneum,' 'cuboides,' 'cuneiform bones,' &c., in order to establish his discovery that a hand and foot are all one.

It is true that if the homological descriptions in the 'Leçons d'Anatomie Comparée' had been quoted as well as the zoological definitions from the 'Règne Animal,' the immortal author of the latter work would be shown to have had previous possession of the homological knowledge. Nay more, in the "Cinquième Leçon, Articles VII.–IX. 'Des os du pied'" *, the frame of the hind feet of Man, Ape, Lion, Seal, Elephant, &c. is shown to consist of homologous bones. Nevertheless the great Zootomist, in his labour and character as Zoologist, does not hesitate to define and differentiate the 'foot,' the 'hand,' the 'paw,' the 'fin,' and the 'hoof,' respectively: nor does he deem the demonstration of the unity underlying the diversity to make the 'man' an 'elephant' or a 'seal,' any more than it makes him a 'dog' or an 'ape'!

It is time that the procedure be exposed and stigmatized which consists in representing the homological knowledge and opinions of the author by his definitions in a purely zoological work, and in suppressing all reference to the descriptions and statements in the anatomical writings of the same author, where his actual knowledge and opinions on the nature and homology of parts are given, and where alone they can be expected to be found.

My present remarks refer to the published 'Abstract' of Mr. Flower's paper. What justice he may have done me by other references in the paper itself, I know not, nor does it concern me since the distribution of

* Leçons d'Anat. Comp., tom. i. 1805.

Part No. 72 of the Proceedings of the Royal Society. In this 'Abstract' I find that, to previous knowledge, and especially my own as represented by the citation of a cerebral character from my Zoological Essay, given at p. 71, Mr. Flower proposes to add, as discoveries of his own, and by way of correction of alleged errors of mine, that the corpus callosum does exist in the Marsupialia and Monotremata; that the transverse fibres connecting the hippocampi are not parts of the fornix, "which is essentially a longitudinal commissure"; that the "two halves of the cerebrum are not by any means disconnected, as the term 'Lyencephala' would imply, but that they are united in a remarkable manner by the immense size of the anterior commissure" (p. 73).

After these pretensions, put forward by a Fellow of the Royal Society in opposition and assumed superiority to a previous labourer in the field, it would naturally be taken for granted that the statements and opinions of the author assailed had been fairly and fully quoted. It might be long before any one would deem it necessary to test the grounds of dispute by reference to Prof. Owen's anatomical writings on the subject. I therefore beg to leave to quote from those writings the following passages.

With regard to the Marsupialia, I state, "This commissure [viz. the commissure of the hippocampi] may nevertheless be regarded as representing, besides the fornix, the rudimental commencement of the corpus callosum." (Phil. Trans. 1837, p. 91.)

In a subsequent anatomical Monograph I wrote, in 1840, "The essential function of the fornix, as a longitudinal commissure, uniting the hippocampus major with the olfactory lobe of the same hemisphere, is more exclusively maintained in the Ornithorhynchus, in consequence of the smaller size of the transverse band of fibres uniting the opposite hippocampi, and representing the first rudiment of the corpus callosum, as it appears in the development of the placental embryo." Art. *Monotremata*, Cycl. of Anat. and Physiol., vol. iii. p. 383, 1841.)

In reference to other connexions of the opposite hemispheres I state, "The anterior commissure is very large in the Monotremes, as in the Marsupials" (*ib.* p. 383); and that "it is the principal commissure of the hemispheres, is subcylindrical, and measured, in the brain of a Platypus, two lines thick vertically, and one and a half lines horizontally" (*ib.* p. 385).

With regard to the inner wall of the cerebral hemisphere, I describe "the fissure upon which the hippocampus is folded" in the Marsupial brain. (Phil. Trans. 1837, p. 90.)

I regret to be compelled to show by the foregoing quotations the sources whence Mr. Flower has derived, or might have derived, the ideas that the essential nature of the fornix, as contradistinguished from its anthropotomical definition, is that "of a longitudinal commissure," that the transverse fibres which connect together the two hippocampi, and form part of the wall of the lateral ventricle, may be regarded as the homologue of part, at least, of the corpus callosum, and that the absence of the main

part of the great commissure of Placentalia is compensated in the Implacentalia by the presence of a large "anterior commissure."

Having no one's shortcomings to exaggerate, I did not, indeed, in the above cited works attribute to this commissure an "immense" size *, but preferred, finding it measurable, to give its dimensions, in the Ornithorhynchus, *e. g.*, and to show, as in fig. 1, *g*, Plate VII. Phil. Trans. 1857, its large proportional size in the Opossum's brain.

I nowhere assert that the mesial wall of the lateral ventricle ('septum ventriculorum' of Mr. Flower) is disconnected with what I affirm to be the beginning of the corpus callosum; on the contrary, both in my original paper in Phil. Trans. 1837, and in the art. *Marsupialia*, I describe that wall or 'septum' to be in part composed by or continued from the superior and internal border of the hippocampal fibres, "forming, in the Wombat, a thin lamina analogous † to the septum lucidum," and, "in the Kangaroo, a stronger and thicker one."

So far as I can comprehend Mr. Flower's account of "the upper and anterior part of the transverse band which passes between the hemispheres of the marsupial brain and radiates out in a delicate lamina above the anterior part of the lateral ventricle," he and I are recording observations of similar facts. Only, inasmuch as the fibres which radiate from the hippocampal commissure to form a delicate lamina above the anterior part of the lateral ventricle, contribute, according to my observations, to constitute part of the wall of such ventricle, and, indeed, a greater proportion thereof than its mere anterior part, I should not describe them as being, or as passing, "between the hemispheres."

The question that remains is the one of interpretation, whether, viz., in reference to the placental condition of the great transverse commissure, and to its relation to the "septum ventriculorum," any portion of the fibres from the hippocampal commissure cross from hemisphere to hemisphere above that septum, after the manner of a 'corpus callosum'?

It is of course open to any anatomist to limit the definition of the fornix, as suggested in my description of the brain of the Ornithorhynchus, to the longitudinal commissural fibres of the hemispheres, and to expand the definition of the 'corpus callosum' to the transverse commissural fibres of the same hemispheres.

Accordingly, when Mr. Flower asks, "granted that only the psalterial fibres are represented in the upper commissure of the Marsupial brain, why should the name of 'corpus callosum' be refused to it?" (p. 73), having shown that no such refusal can be imputed to me, I reply by another question—"Granted that the chief inter-hemispheral connecting fibres in

* "The two halves of the cerebrum are by no means 'disconnected,' the want of the upper fibres is compensated for in a remarkable manner by the immense size of the anterior commissure." (Flower, *loc. cit.* p. 73.)

† At that date (1836) the terms 'analogous' and 'homologous' had not settled significations.

Marsupial brains are homologous with the 'anterior commissure,' why should the name of 'corpus callosum' be refused to them?"

"These fibres [as Mr. Flower repeats after me] are part of the great system of transverse fibres bringing the two hemispheres into connexion with each other," &c.*

But however germane such speculations may be to Philosophical Anatomy, they are altogether out of place in plain zoological definitions. In these, to be of use or to be understood, we must adopt Linnæan sharply-defined terms, such as 'bract,' 'spath,' 'sepal,' 'petal,' 'paw,' 'foot,' 'fin,' 'hand.' If the zoologist believes that he has found characters in the brain leading to an improved classification of a group, he must enunciate those characters in terms by which they will be understood, agreeably with the current and accepted anatomical definitions of the part. It may be long ere either my homological notices or my successors' lead Anthropotomists to dissociate the 'psalterial fibres' or 'transverse commissure of the hippocampi' from the rest of their complex idea of a 'fornix,' or compel them to change the definition of that part of the brain.

No amount of subtle suggestions of signification of delicate radiations of fibres or laminæ will make the 'hippocampal commissure' of the Wombat equivalent to the 'corpus callosum' of the Beaver, in the eye of the naturalist: if the essential element of his idea of a 'corpus callosum' be a mass of transverse fibres crossing the hemispheric fissure, and he does not find them there on divaricating the hemispheres, he will not see them elsewhere at anybody's bidding.

If a group of mammals want such commissural fibres, and another group possess them, the classifier will avail himself of a well-defined term expressing such difference, without prejudice to his reception of any homological determination of the parts, or their rudiments, in anatomical works of the applier of the term.

Finally, I submit the following contrast. Mr. Flower represents the sum of "the literature of the subject" of his paper (p. 71) as "a statement by Professor Owen (Phil. Trans. 1837) of the absence in the Marsupials of the 'corpus callosum,'" and he opposes to that statement "the result of his present investigation" (*ib.*). MM. F. Cuvier and Laurillard, in their description of the marsupial brain, in the posthumous edition of the 'Leçons d'Anatomie Comparée,' sum up my contribution to the literature of the subject as follows:—L'observation de M. Owen sur cette disposition du cerveau des marsupiaux a été repoussée à tort comme erronée. Il ne nie pas l'existence du 'corps calleux' dans les marsupiaux, comme on l'a supposé; il déclare formellement qu'on peut voir, si on le veut, dans ce qui reste de la commissure, le rudiment d'un 'corps calleux'; mais il relève avec raison l'absence dans les marsupiaux d'un 'corps calleux' comparable à celui des autres mammifères."—Vol. iii. p. 101, 8vo, 1845.

* Proc. Roy. Soc. vol. xiv. p. 73.

March 30, 1865.

Major-General SABINE, President, in the Chair.

The Right Honourable Lord Justice Turner, and the Right Honourable the Earl of Donoughmore were admitted into the Society.

The following communications were read:—

I. Reply to Prof. Owen's Paper "On Zoological Names of Characteristic Parts and Homological Interpretations of their Modifications and Beginnings, especially in reference to Connecting Fibres of the Brain," read before the Royal Society March 23, 1865. By W. H. Flower, F.R.S. Received March 28, 1865.

As the above-cited paper consists mainly of complaints of omissions and misrepresentations alleged to be contained in the abstract of my paper "On the Commissures of the Cerebral Hemispheres of the Marsupialia and Monotremata, as compared with those of the Placental Mammals" (Proceedings of Royal Society, vol. xiv. p. 71), I trust that I may be allowed a few words in reply. My first impression on hearing the paper read was a feeling of extreme surprise. When it had become necessary to give publicity to the results of observations which in some respects differed from those recorded by Prof. Owen, I was most anxious, in consequence of the natural respect which I felt for one who has laboured so long and assiduously in the field of anatomical research, that this should be done with the greatest possible deference to his opinions and feelings, and with the smallest semblance of anything which could be construed into an "attack." In this I believed that I had succeeded. That Prof. Owen should have read my "Abstract" from a point of view so different from what I had intended, is to me a source of great regret.

In the brief space allowed for the abstracts of papers communicated to the Royal Society, copious and detailed references to the writings of previous authors are necessarily out of place. Where it is usual only to give an outline of the scientific facts advanced in the paper, it would be obviously improper to follow out the labyrinths of bygone discussions on intricate questions of priority, of definitions, interpretations, and such like matters. Hence most of the citations to be found in my *paper*, not only from the writings of Prof. Owen, but also from numerous other authors, are omitted in the *abstract*. To Prof. Owen's complaints that I have not assigned to him the merit of this or that particular discovery, my reply is that I did this generally once for all in my statement that "at the outset a confirmation is afforded of the important fact, first observed by Prof. Owen, that the brains of animals of the orders Marsupialia and Monotremata present certain special and peculiar characters, by which they may be at once dis-

tinguished from those of other mammals," &c. I never thought it would be attributed to me that I wished it to be believed that every particular statement to which I did not attach the name of some other author was my own original discovery.

Little scope as there is in an abstract for entering into the literature of the subject, a reference to the writings of an anatomist who has contributed so much to advance our knowledge in the special department treated of in my paper could not be altogether omitted. I therefore found it necessary to give in a few words an epitome of the results of those writings. It is in the outline I thus gave that I am acused of serious misrepresentation.

Prof. Owen's first direct charge is contained in the following sentence:—

"It is time that the procedure be exposed and stigmatized which consists in representing the homological knowledge and opinions of an author by his definitions in a purely zoological work, *and in suppressing all reference to the descriptions* and statements in the anatomical writings of the same author, where his actual knowledge and opinions on the nature and homology of parts are given, and where alone they can be expected to be found. My present remarks refer to the published 'Abstract' of Mr. Flower's paper."

To this I reply that my first and indeed only reference in the body of my 'Abstract' is not to any purely zoological work, but to Prof. Owen's original detailed anatomical paper (Phil. Trans. 1837), to which he has himself always referred as containing the amplest exposition of his views upon the subject.

It is next objected that, in quoting what seemed to me the pith and marrow of that memoir, I omitted following passage:—

"This commissure [viz. the commissure of the hippocampi in the Marsupialia] may nevertheless be regarded as representing, besides the fornix, the rudimental commencement of the corpus callosum."

There certainly appeared to me little necessity for the formal citation of a single passage like this, which, if it can be construed into a statement that the corpus callosum is present in the marsupial animals, is perfectly inconsistent with the whole of the remainder of Prof. Owen's memoir, nay further, is immediately contradicted by the context, the whole paragraph standing thus in the original.

"This commissure may nevertheless be regarded as representing, besides the fornix, the rudimental commencement of the corpus callosum; *but this determination does not invalidate the fact* that the great commissure which unites the supraventricular masses of the hemispheres in the Beaver, and all other placentally developed Mammalia, and which exists in addition to the hippocampal commissure, is wanting in the brain of the Wombat; and as the same deficiency exists in the brain of the Great and Bush Kangaroos, the Vulpine Phalanger, the Ursine and Manges Dasyure, and the Virginian Opossum, it is most probably characteristic of the marsupial division of Mammalia." In the same page of the memoir the following occurs:—

"Meanwhile their agreement in so important a modification of the cerebral organ as the absence of a corpus callosum and septum lucidum, affords additional and strong grounds for regarding the Marsupialia as a distinct and peculiar group of mammals." Notwithstanding this clear and definite statement, which occurs again and again in some form or other throughout the memoir, we are told that it is misrepresentation to quote Prof. Owen as alleging "the absence in the marsupials of the corpus callosum." But Prof. Owen has failed to notice that the discussion of the homological relations advanced in the above cited short passage, and in similar terms in his articles in the 'Cyclopædia of Anatomy,' is by no means passed over even in my abstract, as the paragraph commencing with the following words will show.

"Can this transverse commissure, of which the relation is so disturbed by the disposition of the inner wall of the hemisphere, be regarded as homologous to the entire corpus callosum of the placental mammals? or is it, as has been suggested, to be looked upon as only representing the psalterial fibres or transverse commissure of the hippocampi?" (Proc. Roy. Soc. vol. xiv. p. 72.) If, after the words "as has been suggested," I had added "by Prof. Owen," there would, I believe, have been nothing wanting to complete as fair and full an exposition of that author's views as was compatible with the limits of an abstract.

I now regret the omission. I thought that as Prof. Owen's name occurred, both before and after, in connexion with the subject, and as no other author was mentioned, it would easily be surmised that the suggestion was his. I was moreover, as I stated before, especially anxious to avoid giving a polemical appearance to my paper by too frequent citations by name, where it was necessary to show a divergence of opinion.

I now come to the alleged misrepresentation of Prof. Owen's opinions contained in a foot-note to my abstract, which runs as follows:—

"In the paper by the same author [Prof. Owen] 'on the Characters, Principles of Division, and Primary Groups of the Class Mammalia' (Proc. Linn. Soc. 1858), the Subclass *Lyencephala* ('loose' or 'disconnected' brain), equivalent to the Marsupialia and Monotremata, are characterized as having the cerebral hemispheres but feebly and partially connected together by the 'fornix' and 'anterior commissure,' while in the rest of the class a part called 'corpus callosum' is added, which completes the connecting or commissural apparatus." It is now objected that this was only intended as a "zoological definition" or "character."

Not being aware that a zoological character, valid as such, can misrepresent an anatomical truth, when I wished to find a brief epitome of Prof. Owen's latest views upon the nature of the commissures of the marsupial brain, in corroboration of the one I had given in the text from his earliest memoir, I adopted the above statement. I adopted it, moreover, because he had himself referred to it in the following emphatic terms. I quote from the well-known "Reade Lecture," delivered before the University of Cam-

bridge in 1859 * (p. 23): "At length, having dissected the brain, in one species at least, of almost every genus or natural family of the mammalian class, I felt myself in a position to submit to the judgment of my fellow labourers in Zoology, at the Linnean Society in 1857, the generalized results of such dissections, comprising a fourfold primary division of the MAMMALIA based upon the four leading modifications of cerebral structure in that class.

"In some mammals the cerebral hemispheres are but feebly and partially connected together by the 'fornix' and 'anterior commissure'; in the rest of the class the part called 'corpus callosum' is added, which completes the connecting or 'commissure' apparatus.

"With the absence of this great superadded commissure is associated a remarkable modification of the mode of development of the offspring. * * * This first and lowest primary group, or subclass of Mammalia, is termed, from its cerebral character, LYENCEPHALA, signifying the comparatively loose or disconnected state of the cerebral hemispheres."

I think that I should scarcely be blamed for putting my trust in an author's own description of the "generalized results" of his researches, deliberately laid before his fellow-labourers at a meeting of a learned Society twenty years after those researches were made.

I may add, moreover, that the works, both English and foreign, upon Comparative Anatomy and Physiology, in which the simple fact that the marsupials and monotremes differ from the other mammalia by the absence of a corpus callosum or great transverse commissure to their brain is stated upon the authority of Prof. Owen, may be said to comprehend most of those of any importance published since the year 1837. One or two examples will suffice. MM. Eydoux and Laurent (Voyage de la Favorite) have thrown into a tabular form the published results of the dissections of the brains of the Implacental Mammalia as compared with placental mammals and birds, in which Table the part played by the corpus callosum is as follows:—

	MONODELPHES.	DIDELPHES.	ORNITHODELPHES.	OISEAUX.
"Corps calleux...	existe.	manque.	manque.	manque."

This statement of the "résultat des observations de M. R. Owen," so far from having called forth the strictures of that anatomist, is quoted with approbation in his article "Monotremata" in the 'Cyclopædia of Anatomy and Physiology.'

In Van der Hoeven's 'Zoology,' vol. ii. p. 596 (Dr. Clark's edition), it is stated that "the great transverse commissure of the hemispheres of the cerebrum is, with the exception of the Monotremes and Marsupiates (R. Owen, 'On the Structure of the Brain in Marsupial Animals,' Phil. Trans. 1837), present in all mammals." In the preface of the same work, the editor, speaking upon the authority of the above-mentioned paper in the Journal of the Linnean Society, says, "In some mammals the cerebral

* On the Classification and Geographical Distribution of the Mammalia, 1859.

hemispheres are only partially connected by the fornix and the anterior commissure; in the rest of the class the corpus callosum is added."

I will only cite one other instance to show that I am far from being singular in the outline I have given of the most important part of Professor Owen's researches upon this subject.

In the year 1851 the Copley Medal of this Society was awarded to Professor Owen. In the epitomized account of his labours, the observations upon the structure of the Marsupial Brain are referred to in the following terms. I quote from the President's Address (Proceedings of the Royal Society, Dec. 1st, 1851):—"In the Philosophical Transactions for 1837 appeared a memoir from Professor Owen's pen, describing certain peculiarities in the brain of the Marsupialia, *especially the absence of the* corpus callosum. The same condition he subsequently discovered in the Ornithorhynchus and Echidna. This, and other peculiarities of structure in the sanguiferous, osseous, and dental systems, led Professor Owen to suggest a modification of the classification of the mammalia which Cuvier had adopted in his last edition of the 'Règne Animal.' Deeming modifications of brain of more importance than those of ungual phalanges, and connecting the higher development of the commissural system of the brain with the longer sojourn of the fœtus in the womb, and its more intimate union therewith, Professor Owen, in his paper 'On the Classification of the Marsupials' in the Trans. Zool. Soc. 1839, groups together all the mammalia which have a placenta under any form, and which have a corpus callosum, in a primary *subclass,* under the name of Placentalia; the rest form the Subclass *Implacentalia,* and this includes the orders Marsupialia and Monotremata."

This outline of the most important results of a series of anatomical researches could not have been intended as a "zoological definition." If I have misrepresented Professor Owen in stating broadly that he alleged that the implacental mammalia were distinguished from the remainder of the class by the absence of the corpus callosum, so also did the compiler of the above paragraph; and at least one of the reasons assigned for the award of the highest honour the Society can bestow is grounded on a misconception. The main difference as to questions of fact between Professor Owen and myself may be stated briefly as follows. He has seen and described, in the brains of the implacental mammals, a transverse commissure between the hemispheres of the cerebrum, which he calls 'fornix,' 'commissure of the hippocampi,' or sometimes 'rudimental commencement of the corpus callosum.' This commissure he appears to have seen (or at all events to have described) only in a portion of its extent. He has found in placental mammals a great transverse commissure superadded to this, to which he generally restricts the term 'corpus callosum.'

I have seen and described in the brain of several implacental mammals the same transverse commissure, but have traced out its relations and connexions more fully, especially by means of transverse and lon-

gitudinal sections. If my determination is correct, it represents (not merely according to transcendental homological signification, but as a simple question of plain anatomical observation) the whole of the great transverse commissure, or corpus callosum of the lower placental mammals, only in somewhat reduced proportions, and with relations somewhat modified by the peculiar form of the inner cerebral wall.

There is consequently no superadded structure in the brain of the latter group.

To the imputation, twice repeated, of having "obtained" or "derived" the notions and ideas contained in my paper from Professor Owen's writings, no direct reply is necessary. The communication which I presented to the Society is the result of repeated original observations and dissections, made at various periods, extending over more than three years. The descriptions are all verified by drawings and preparations.

That their publication (if they should be so honoured) may advance in some slight degree our knowledge of a difficult and obscure, yet important branch of anatomy, is all that I venture to hope. That they are entirely free from errors, or that they may not, at some future time, be superseded by the researches of abler investigators, I do not presume to believe.

II. "On the Size of Pins for connecting Flat Links in the Chains of Suspension Bridges." By Sir Charles Fox. Communicated by the President. Received March 2, 1865.

In the construction of chains of this kind, it is of the highest importance that the pins, which pass through and connect together the links of which the chains are composed, should be of the right size, inasmuch as their being too small, as compared with the links through which they pass, renders ineffective a portion of the iron contained in the latter, which then exists only as a useless load to be carried by such links; while at the same time, if the pins and heads of the links be too large, they become uselessly cumbersome and expensive.

Careful examination and experiments made upon a large scale (which will be explained hereafter) have brought out facts by which a simple rule has been arrived at—a rule that may safely be adopted as a guide in deciding upon the relative sizes of these two parts.

On this rule mainly depends the economical use of iron in the construction of such chains.

In this paper the term chains for suspension bridges implies such as are usually employed, and are composed of several flat bars of equal thickness throughout, placed side by side, but having their ends swelled edgeways so as to form what are technically termed heads, and which are coupled together by pins passing through holes in such heads, as shown in figs. 5 & 6 in the accompanying drawing.

In deciding upon the size of the pins, it has often been assumed, as

a close approximation, that, as about the same force is required for shearing as for breaking wrought iron by extension, it would be necessary to give the pin a cross section equal to the sectional area of the smallest portion of the link only. The fact of the possibility of links being torn and destroyed by the pin being too small to present the necessary bearing surface, although quite large enough to resist the calculated shearing force brought to bear upon it by the links, seems hitherto not to have attracted notice; but as the strength of a chain depends upon the proper extent of surface being offered by the pins of the links to pull against, such a mode as the one described has been proved by experiment to be altogether fallacious. For by this mode of estimating, the size of a pin passing through links 10 inches wide and of uniform thickness (that is, not having the head thicker than the body of the link) would be something less than $3\frac{1}{2}$ inches in diameter, whereas (as will presently be shown), in order to get the whole benefit from such a link, the pin must be somewhat more in diameter than 6 inches, and for the following reasons.

In wrought iron the initial forces necessary to extend, or diminish by compression, the length of a bar are practically the same; and hence it arises that unless the surface of the pin on which the semicylindrical surface of the hole in the link bears is as great as the smallest cross section of the link itself, the head will be torn by the pin; and since to provide this necessary surface it is essential to have a pin of much larger size, the question of its ability to resist the operation of shearing never arises, and the whole subject resolves itself into one of bearing surface.

If the pin be too small, the first result on the application of a heavy pull on the chain will be to alter the position of the hole through which it passes, and also to change it from a circular into a pear-shaped form (*vide* fig. 2), in which operation the portions (A A, figs. 2 & 3) of the metal in the bearing upon the pin become thickened in the effort to increase its bearing surface to the extent required. But while this is going on, the metal around the other portions (B B, figs. 2 & 3) of the hole will be thinned by being stretched, until at last, unable to bear the undue strains thus brought upon it, its thin edge begins to tear, and will, by the continuance of the same strain, undoubtedly go on to do so until the head of the link be broken (or, rather, torn) through, no matter how large the head may be; for it has been proved by experiment that by increasing the size of the head, without adding to its thickness (which, from the additional room it would occupy in the width of the bridge, is quite inadmissible), no additional strength is obtained.

Acting upon the principle above described, most engineers have made the pins of their chains far too small, whereby much money has been wasted in making the links of a size, and consequently of a strength, of which it was, through the smallness of the pins, impossible to obtain the full benefit. Indeed to such an extent has this been carried, that in one of the most noted suspension bridges hitherto constructed, a very large sum

has been thrown away upon what is worse than wasted material, inasmuch as that material, remaining as load only, has to be carried by the chains, and correspondingly weakens the structure.

I am also acquainted with a very recently constructed suspension bridge in which some of the links, which are 10 inches wide, have the holes in their heads but 2 inches, instead of $6\frac{1}{2}$ inches, in which case more than two-thirds of the iron in the links is useless.

The first time my attention was seriously called to this important subject was when Mr. Vignoles entrusted my late firm of Fox, Henderson, and Co., with the manufacture of the chains of the great suspension bridge for carrying a military road over the Dnieper, at Kieff, which was constructed by him for the Russian Government.

As the chains for this bridge weighed upwards of 1600 tons, upon which the expense of transport was very heavy, they having to be shipped to Odessa, and thence carted over very bad roads for upwards of 300 miles to Kieff, it was considered of the first importance to ascertain whether or not they were well proportioned; and accordingly a proving-machine was specially prepared, of power sufficient to pull into two any link intended to be used on this bridge.

These links, as shown in the drawing attached to the contract (see fig. 1), were, for convenience of transit, but 12 feet long from centre to centre of pin-holes, $10\frac{1}{4}''$ wide by $1''$ thick in their body or smallest part, with a head at each end also $1''$ thick, swelled out to $16\frac{1}{2}''$ in width, so as to allow of holes for receiving pins $4\frac{1}{2}''$ in diameter. The cross-sectional area of these pins was 15·9 inches, or rather more than 50 per cent. in excess of the cross-sectional area of the link at its smallest part.

According to the usual mode of ascertaining the size of these pins, by making them of such dimensions as to resist the force required to shear them, they possessed upwards of a third more section than was thus shown to be necessary. Still, in practice, a pin of this size proved altogether disproportionate to the size of the links, and required to be increased from $4\frac{1}{2}''$ to $6\frac{1}{2}''$ diameter before it was possible to break a link in its body or narrowest part—fracture in every previous case taking place at the hole, and through the widest part of the head, as shown in fig. 2.

The iron in the links for this bridge was of a very high quality, and was manufactured by Messrs. Thorneycroft and Co., from a mixture of Indian and other approved pig-iron, and required a tensile strain of about 27 tons per sectional inch to break it; so that taking the narrowest part at, say 10 inches, a strain of 270 tons ought (had the size of the pin been in proper proportion) to have been required to pull it into two; instead of which, so long as the pins were but $4\frac{1}{2}$ inches in diameter, the head tore across (as shown at fig. 2) at its widest part with about 180 tons, or two-thirds only of the strain desired and provided for as far as the size of the body of the links was concerned.

This unexpected result led to the belief that the size of the heads was

insufficient; and accordingly a few experimental links were prepared with their heads 2 inches wider than before (as shown in fig. 4); but these nevertheless were found to require no additional force to tear asunder; hence it became obvious that fracture arose from some cause not yet ascertained.

As has already been stated, the rupture took place across the widest part of the head (C C, fig. 2); but on attempting to adjust the piece broken off to the position it originally occupied in the link, it was observed that, while the fractured surfaces came in contact at the outside of the head, they were a considerable distance apart at the edge of the pin-hole (see fig. 2).

This at once proved that during the application of the tension, which at last ended in producing fracture, the various portions of the head had been subject to very unequal strains; and upon careful examination, the rationale of this fracture became apparent from the consideration that the hole, which originally was round, had become pear-shaped (see fig. 2), having altered its position, and that the iron of the link which, during the application of the load, bore upon the pin, and was consequently in a state of compression, had become considerably thickened in consequence, as was now evident, of an effort to obtain a greater bearing surface (see A, figs. 2 & 3), while the other portion of the iron around the pinhole, being subject to tension, had been so weakened and thinned by being stretched, as to cause a tearing action to take place, which, having once commenced, would obviously, by the continuance of the same strain, rend through the entire head, no matter what its width might be.

From this it was clear that any increase of size in the head (unless by thickening, which, as I have before stated, is inadmissible) was of no avail; and it was now that the principle which forms the subject of this paper became manifest—viz., that there was a certain area of the semicylindrical surface of the hole having a bearing on the pin proportionate to the transverse section of the body or narrowest part of a link, and quite essential to its having equal strength in all its parts; and that any departure from this proportion could not fail to bring about either waste of iron in the body of the links, if the pin were of insufficient size to offer proper bearing surface, or waste of metal in the heads of the links and in the pins, if the latter were larger than necessary for obtaining this fixed proportion of areas.

Having arrived at this point, a link, similar in all respects to the previous one, with holes $4\frac{1}{2}$ inches in diameter, and which broke across the head with 180 tons, was taken, and its holes enlarged to 6 inches, but without increasing the width of the head, which still remained $16\frac{1}{2}$ inches; so that the only difference was the removal of an annular piece $\frac{3}{4}$ inch in width from the hole, and so making it 6 inches instead of $4\frac{1}{2}$ inches in diameter, thereby actually diminishing the quantity of iron in the head to this extent—when it was most interesting to discover that by this slight alteration, by which the semicylindrical surface bearing on the pin had been increased

from 7·0 to 9·4 sectional inches, the power of the link to resist tension had increased in about the like proportion, having rendered a force of nearly 240 tons necessary to produce fracture.

Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5. Fig. 6.

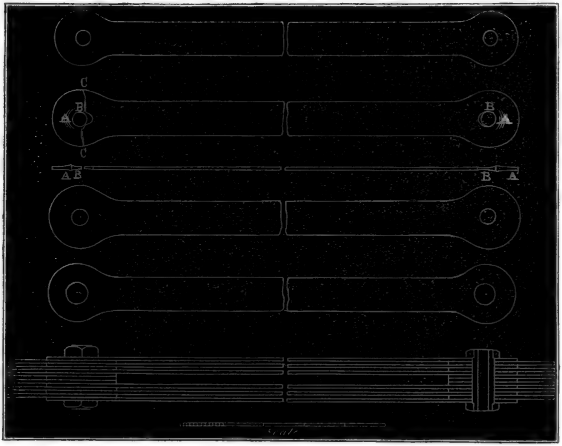


Fig. 1. Link for Kieff Bridge. Depth of head $16\frac{1}{2}$ in., of centre $10\frac{1}{4}$ in., diameter of hole $4\frac{1}{2}$ in.

Fig. 2. Elevation showing result of proof.

Fig. 3. Section through centre, showing result of proof.

Fig. 4. Experimental link, with wider head. Depth of head $18\frac{1}{2}$ in., diameter of hole $4\frac{1}{2}$ in.

Fig. 5. Link with properly proportioned hole for pin. Depth of head $17\frac{1}{2}$ in., diameter of hole $6\frac{1}{2}$ in.

Fig. 6. Plan of chain, and section of pin and links.

From subsequent experience, it has become evident that had the pins of these chains been increased to $6\frac{1}{2}''$ diameters, giving a bearing surface of 10·2 square inches, the proper proportion between them and the body of the links would have been very nearly arrived at, while with those of only $6''$ diameter about an inch of the body of the links was wasted.

The practical result arrived at by the many experiments made on this very interesting subject is simply that, with a view to obtaining the full efficiency of a link, the area of its semicylindrical surface bearing on the pin must be a little more than equal to the smallest transverse sectional area of its body; and as this cannot, for the reasons stated, be obtained by increased thickness of the head, it can only be secured by giving a sufficient diameter to the pins.

That as the rule for arriving at the proper size of pin proportionate to

the body of a link may be as simple and easy to remember as possible, and bearing in mind that from circumstances connected with its manufacture the iron in the head of a link is perhaps never quite so well able to bear strain as that in the body, I think it desirable to have the size of the hole a little in excess, and accordingly for a 10″ link I would make the pin $6\frac{2}{3}''$ in diameter instead of $6\frac{1}{2}''$, that dimension being exactly two-thirds of the width of the body, which proportion may be taken to apply to every case.

As the strain upon the iron in the heads of a link is less direct than in its body, I think it right to have the sum of the widths of the iron on the two sides of the hole 10 per cent. greater than that of the body itself (see fig. 5).

As the pins, if solid, would be of a much larger section than is necessary to resist the effect of shearing, there would accrue some convenience, and a considerable saving in weight would be effected, by having them made hollow and of steel.

In conclusion, I would remark that my object in writing this paper has been, first, to call attention to the fact that a link is far more likely to be torn by the pin being too small, than a pin to be sheared by a link; and secondly, to try to establish a simple rule by which their proper comparative sizes may always be arrived at; and I have been induced to investigate this very important subject from having generally found in existing suspension-bridge chains a wide departure from what is right in this respect, in having the pins far too small.

III. "On the Influence of Quantity of Matter over Chemical Affinity, as shown in the formation of certain Double Chlorides and Oxalates." By George Rainey, M.R.C.S., Lecturer on Microscopical Anatomy, and Demonstrator of Surgical Anatomy at St. Thomas's Hospital. Communicated by Dr. Gladstone. Received March 2, 1865.

The simple fact that quantity of matter has the effect of influencing chemical affinity is so well known and so generally admitted, that any special remark upon it would be superfluous; I shall therefore in this communication chiefly confine my observations to the compounds above named, by which this effect will be shown to be strikingly exemplified, offering such explanations and remarks thereon as the nature of the facts may seem to demand.

The results of nearly all the experiments mentioned in this paper were first arrived at by operating upon very minute quantities of material, and by observing under the microscope the changes that take place; but afterwards the same products were obtained on the large scale by appropriate processes, and in quantities sufficiently large to admit of being analyzed quantitatively, and of having their formulæ accurately determined. I shall therefore commence by giving an account of the processes by which the

various compounds I am about to treat of can be most easily prepared in minute quantities, and afterwards those by which they can be formed in any quantity that may be required.

All the apparatus necessary for the preparation of these compounds in quantities sufficient for microscopical examination, are a few cells made by cementing a ring of thin glass, such as is ordinarily used by microscopists, and some circular disks of thin glass to be employed as covers. The cell must be sufficiently shallow to allow of the examination of its contents with a lens of a half or quarter of an inch focus.

To prepare the compound of oxalate and chloride of strontium, introduce into a cell a few crystals of oxalate of strontia, and add to them as much saturated solution of chloride of strontium as will completely fill the cell; then cement upon the cell a thin glass cover in such a way that the cell shall be completely air-tight. A cell thus charged must be kept in a horizontal position, and examined from time to time. In a few hours the angles and edges of the oxalate of strontia crystals will be observed to have lost their sharpness of outline, being in a state of disintegration, and very minute crystals, altogether of a different form from those of the oxalate, to have made their appearance; and on successive examinations all the octahedral crystals will be seen to have disappeared, and to have become replaced by exceedingly well formed rhomboidal crystals of different sizes, composed of the two salts introduced into the cell. (Fig. 1.) In this ex-

Fig. 1.

periment the solution of the chloride of strontium must be fully saturated; and if it even contain a few undissolvod crystals, the processes of disintegration of the oxalate, and formation of the compound, will be prolonged, and more time allowed for their microscopical examination.

If, in the place of oxalate of strontia and chloride of strontium, crystals of oxalate of lime, and a strong solution of chloride of calcium be introduced into a microscope-cell, and the cell be closed up, the crystals of this oxalate (like those of the oxalate of strontia) will be seen by the microscope

to become gradually disintegrated, and replaced by an entirely different set of crystals, consisting of oxalate of lime and chloride of calcium. In this experiment, either the oxalate made artificially, or that obtained from any natural source, may be employed; but the strength of the solution of the chloride of calcium must not be less than that of a mixture consisting of equal parts by measure of a solution of this chloride saturated at a temperature of 60° Fahr. and water. The time required for the disintegration of all the oxalate of lime and its combination with the chloride of calcium to form crystals of the double compound of these salts, depends upon the size of the crystals of oxalate of lime employed in the experiment: if they are very small, or if the oxalate is to all appearance amorphous, evidence of these changes will be visible in a few hours, and the crystals completely formed in a few days; whilst if the oxalate crystals are large and well formed, some weeks will elapse before they have entirely disappeared and become replaced by the new crystals.

These changes are also influenced by the strength of the solution of the chloride of calcium: if it be fully saturated, they take place more quickly, but the crystals will be small; if diluted, they will, on the contrary, be produced more slowly, and the crystals will be larger and better defined. (fig. 2.) Crystals of oxalate of magnesia put into a cell with a saturated

Fig. 2.

solution of chloride of magnesium undergo similar changes, a double salt, consisting of oxalate of magnesia and chloride of magnesium, being formed. In this case the changes take place very slowly, several months being required for the formation of perfect crystals. These crystals have their sides and angles exceedingly sharp and well defined (fig. 3). Lastly, the oxalate of baryta, and a saturated solution of the chloride of barium placed under the conditions above mentioned, yield the same results, and crystals of oxalate of baryta and chloride of barium are formed (fig. 4.)

The series of facts requiring notice in the process of formation of these

double salts, as shown in the foregoing experiments, are, first, the disintegration, and at length complete dissolution of a class of salts of very

Fig. 3.

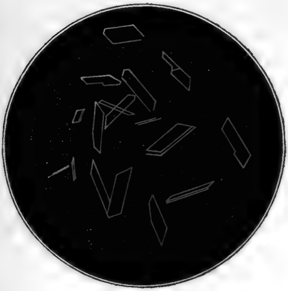


Fig. 4.

sparing solubility, in strong solutions of chlorides of the same base, whilst weaker solutions do not in the least affect them; secondly, the combination under these circumstances of the oxalate and chloride of these bases to form double salts almost as little soluble as the simple oxalates; and lastly, the continued and simultaneous solution and deposition of these salts in a crystalline form in a quantity of fluid but little, if at all, exceeding the weight of the crystals deposited, until either all the oxalate crystals employed at the commencement are used up, or the solution of the chloride has become too feeble to effect the further disintegration of those which are in excess. The most remarkable part of this process is the continued deposition of crystals after the saturation of the fluid in which they were formed, rendering a small portion of fluid sufficient for the production of a comparatively large quantity of crystals. This fact seems to indicate that in this case the newly-formed particles, immediately on their coming into existence in this fluid medium, are attracted more forcibly by the fluid than by one another, but that afterwards, when they have become augmented to a point above that which is said to be the point of saturation, their attraction for one another becomes greater than that for the fluid with which they are in contact; and being thus brought within the sphere of action of the forces necessary to produce crystals, these particles combine into the crystalline form.

Whether under conditions like the above a deposition of crystals is a necessary attendant on the formation of sparingly soluble substances in a fluid medium, it is not possible to say; but in course of these investigations I shall be able to adduce several other examples of a similar kind.

The experiments which have been related so far have gone only to exemplify the influence of quantity of matter on chemical affinity as connected with simple chemical combination, but by a slight modification of

these experiments the effect of the same principle on elective affinity can be demonstrated. For this purpose let a small quantity of oxalate of lime, either amorphous or crystalline, be introduced into a microscope-cell filled with a completely saturated solution of chloride of strontium, and securely closed up. On examining such a preparation, the oxalate of lime is seen in a few days to be undergoing disintegration, and new crystals are seen to make their appearance, generally first at the sides of the cell. These crystals being formed slowly have a very sharp outline, and are remarkably transparent. In their form they resemble those obtained by putting oxalate of strontia into a saturated solution of the chloride of strontium; and notwithstanding that many of them have some of their angles cut off, and are thus made to present additional sides, their composition is precisely the same as if oxalate of strontia had in this instance been acted upon by a saturated solution of chloride of strontium. As in this experiment a chloride of calcium is formed at the expense of some of the chloride of strontium, the solution soon becomes too feeble to decompose the oxalate of lime, when of course the further formation of crystals will cease. Hence, to secure the best results, some crystals of chloride of strontium must be undissolved in the solution when put into the cell. By this means the processes of disintegration and the production of new crystals can be prolonged for many months, and an opportunity is thereby afforded of measuring them, and of determining their rates of increase, or of making such other observations on the subject as the experiment may suggest.

The methods of forming the double compounds of the oxalates and chlorides of the alkaline earths in microscopical quantities having thus been described, it now remains to show how, on the same principle, these compounds can be formed on the large scale.

To prepare the double salt consisting of oxalate of strontia and chloride of strontium, it is necessary merely to mix oxalate of strontia (made by decomposing the oxalate of ammonia by chloride of strontium, and washing the precipitate by decantation, and not on a filter) with a solution of chloride of strontium kept at the point of complete saturation. In order that no lumps of oxalate may be mixed with the solution of the chloride, which would be unavoidable if any portion of the oxalate had been allowed to get dry, it is necessary, after finally washing the oxalate, to draw off as much of the water as possible with a siphon, and to add to the mixture of the oxalate of strontia and remaining water the saturated solution of chloride of strontium containing some undissolved chloride. If the solution of the chloride of strontium in which the oxalate is contained is fully saturated, the combination will begin in a few hours; but it will require two or three weeks before all the oxalate has combined with the chloride. This can be determined only by the microscope.

The double salt of oxalate of lime and chloride of calcium can be formed in the same manner. In the formation of this compound it is not necessary that the solution of the chloride of calcium should be fully saturated; and

the weaker the solution of this chloride is, provided only it is of sufficient strength to combine with the oxalate, the larger will be the crystals of the double salt.

Since writing the above I find that the same double compound of oxalate of lime and chloride of calcium has been prepared by Fritsche by a different process.

Not having formed the double salts of oxalate of magnesia and oxalate of baryta with their respective chlorides on the larger scale, I am not able to give any special directions as to the easiest method of preparing them in large quantities; but from what has been stated concerning the microscopical processes, the mode of preparing them in such quantities is obvious.

Of the general properties of the double salt of oxalate and chloride of strontium, I may observe that it is very slightly soluble in the solution in which it is prepared, but rather more so in the hot than in the cold solution. It can be deprived in a great measure of the fluid in which it was formed, by pressure between sheets of blotting-paper: it is neither deliquescent nor efflorescent. It is decomposed by water into oxalate and chloride, but it is not at all affected by absolute alcohol, being entirely insoluble therein; hence this medium can be employed in freeing the salt from any adherent chloride: to effect this, the washing with absolute alcohol must be done by decantation, and not on a filter, as during the evaporation of the alcohol a small quantity of water is deposited from the air, which would decompose some of this compound salt, and, the chloride of strontium being soluble in alcohol, some oxalate would be left in excess.

For the analysis of this salt I am indebted to the kindness of Mr. Holmes, Dr. Gladstone's assistant, and Mr. Tribe of St. Thomas's Hospital.

The following is the formula deduced from the subjoined analysis of Mr. Holmes:—

$$\mathrm{C_2\,Sr_2\,O_4\,.\,2Sr\,Cl\,.\,2H_2\,O\,.\,4Aq.}$$

	Experiments. I.	II.	Theory.
Oxalate of strontium	39·53	39·63	39·58
Chloride of strontium........	36·02	35·59	35·80
Water, fixed at 100° C.......	8·11	8·25	8·18
Water, given off at 100° C. ..	16·34	16·53	16·44
	100·00	100·00	100·00

which agrees with that deduced by Mr. Tribe.

Besides this there is another compound of oxalate of strontia and chloride of strontium, consisting of different proportions of these constituents, which can be prepared in the following manner:—To well-formed crystals of the above salt, contained in two or three times their weight of saturated solution of chloride of strontium, add gradually, and at intervals of two or three hours, a mixture of equal parts of saturated solution of chloride of strontium and water, until by examination with the microscope the greater

part of the rhomboïdal forms are seen to have disappeared, and to have become changed into crystals of various sizes, and generally of a prismatic shape; after which the mixture is to be set aside for two or three days. If, after the expiration of this time, all the rhomboidal forms have not disappeared and become replaced by prisms, fresh solution must be added until such is the result. The size of the new crystals and their degree of completeness will be in proportion to the time employed in their formation. (Fig. 5 is a representation of these crystals.)

Fig. 5.

This compound of the double oxalate and chloride can also be prepared by keeping the rhomboidal form for three or four weeks in an atmosphere saturated with aqueous vapour, and completely excluded from the external air. By this mode of treatment these crystals in a few hours absorb water, and begin to undergo a process of disintegration, and in two or three days distinct well-defined prismatic crystals make their appearance. These processes of disintegration and formation of new crystals continue until all the rhombs are converted into prisms, which are contained in the water absorbed from the húmid air, in which they may be preserved if kept in a well-stopped bottle, so as to prevent evaporation and the consequent reconversion of the prisms into rhombs. This form of double salt, like the one from which it was prepared, is instantly decomposed, by the sudden addition of a sufficiently large quantity of water, into oxalate of strontia and chloride of strontium. It is not affected by exposure to the air, and it is insoluble in absolute alcohol.

The following is the formula deduced from the subjoined analysis by Mr. Holmes:—

$$3\,C_2\,Sr_2\,O_4\,.\,2\,Sr\,Cl\,.\,4H_2O\,.\,12\,Aq.$$

	Experiments. I.	II.	Theory.
Oxalate of strontium	54·03	54·22	54·15
Chloride of strontium.........	16·44	16·51	16·31
Water, fixed at 100° C........	7·77	7·40	7·39
Water, given off at 100° C. ..	21·76	21·87	22·15
	100·00	100·00	100·00

With respect to the mode in which the change of the first compound into the second takes place under the circumstances above detailed, there is one fact which it is necessary to mention, namely, that the solution of chloride of strontium of the strength in which the prismatic crystals are formed from the rhomboidal, and in which they can be kept unchanged, does not affect the crystals of the simple oxalate. Hence it is evident that the oxalate set free by the decomposition of the rhomboidal crystals by the addition of only a small quantity of water, is, from the circumstance of its being brought into contact with the dissolved chloride of strontium on the instant of its formation, more easily acted upon by it than if it had before been in a perfectly formed state. This is probably from the nascent particles not having assumed a decidedly solid form; the chemical union would not require to be preceded by the same process of mechanical disintegration as in the latter case. The subsequent processes of solution of the incipient particles of the newly-formed salt, and their deposition in a crystalline form, admit of the same explanation as that already given in the case of the microscopic process.

As to the properties of the double salt of oxalate and chloride of calcium, I may observe that this salt is acted upon by water similarly to that of the oxalate and chloride of strontium, being instantly resolved into oxalate and chloride by the sudden action of water in sufficient quantity. The action of water applied gradually and in small quantity I have not sufficiently examined to be able to give a process for the preparation of a second salt. The salt is very slightly soluble in the solution in which it is formed, and rather more so in the hot than in the cold solution. After all the solution of chloride of calcium has been separated from it by means of blotting-paper, or by means of ablution with absolute alcohol, it is not deliquescent in an ordinary atmosphere.

The following is the formula deduced by Mr. Tribe from the subjoined analysis:—

$$\Theta_2\, Ca_2\, \Theta_4 \,.\, 2Ca\, Cl \,.\, 2H_2\Theta \,.\, 5\, Aq.$$

	Experiments. I.	II.	Theory.
Calcium	21·88	21·88	21·92
Oxalic acid	23·67	23·67	24·11
Chlorine	19·40	19·44	19·45
Water, fixed at 100° C.......	10·36	10·14	9·86
Water, given off at 100° C. ..	24·69	24·74	24·66
	100·00	100·00	100·00

Before concluding this part of my subject, I have a few observations to make on the oxalates of the alkaline earths produced by the sudden action of water in sufficiently large quantity upon their double salts, especially those of the double salts of strontia and baryta.

For this purpose it is necessary only to drop a few well-formed crystals of the double salt of oxalate of strontia and chloride of strontium into a test-tube filled with distilled water, when they will be seen instantly to become opaque. The general form of these crystals remains more or less complete, but their interior is filled up with entirely amorphous oxalate, especially in the very thin crystals. If some of the crystals thus acted upon be put into distilled water in a microscope-cell perfectly closed to prevent the escape of the fluid by evaporation, and examined from time to time with a $\frac{1}{4}$-inch lens, they will be seen gradually to go into well-defined octahedral crystals, some months being required before the whole of the amorphous oxalate is converted into crystals; but all specimens thus prepared are not acted upon by water to the same degree. Among several slides showing this fact, I have one in my possession dated May 20th, 1863, in which the whole of the amorphous oxalate introduced has become transformed into remarkably beautiful and well-formed octahedral crystals, many of them of large size, considering that they had been formed in so small a quantity of water—the mere fraction of a drop. Some portions of this oxalate remained unchanged for more than a year after it was put into the cells, but the whole of it has since been converted into perfect crystals. As the effects of water upon these forms of oxalate are not uniform, it was considered necessary first to determine whether it was only on the oxalate as above prepared that water caused these changes, or whether it was not a general consequence of the action of water upon sparingly soluble salts, rendered amorphous in consequence either of a complete absence of water, or of an insufficiency for perfect crystallization (the sudden action of the water upon the double salt separating the chloride from the oxalate more rapidly than the water of crystallization could combine with it); and with this view the following experiments were performed. The oxalates of lime, strontia, and baryta, all made by precipitation in the usual way, were exposed, for the purpose of expelling the water of crystallization, in small test-tubes to the temperature of boiling oil for three or four hours, and afterwards put into closed microscope-cells with distilled water. On examination with the microscope, it was found that the oxalate of baryta had begun in a few hours to pass into a more minute state of division, and that on the same day octahedral crystals could be seen by a $\frac{1}{4}$-inch lens; and in the space of six weeks nearly all this oxalate had become transformed into regular octahedral crystals. The oxalate of strontia had undergone a similar change, but rather more slowly; distinct octahedral crystals, however, were visible in twelve hours.

As the oxalate of strontia obtained by precipitation in the usual way generally contains some octahedral crystals, some of the amorphous oxa-

late employed in this experiment had been prepared by exposing to the action of boiling oil an amorphous oxalate of strontia, precipitated from its combination with chloride by means of water. This precipitate not containing any octahedral crystals, and being acted upon more speedily by water than the other forms of anhydrous oxalate of strontia, is best suited for the demonstration of the effect above described. The oxalate of lime which had been subjected to the action of boiling oil and kept in distilled water had not undergone any visible change. I may notice, in reference to some oxalate of strontia which had been heated in boiling oil and exposed to the air for five or six weeks, that the action of water upon it was diminished, but was entirely restored after a second exposure to the heated oil.

In reference to the cause of these changes from the amorphous to the crystalline form by the mere action of water, it is evident that, as they take place in perfectly closed cells, and commence in so short a time after the introduction of the amorphous oxalate and water, they are independent of evaporation or of alteration of temperature. Hence they seem to be simply the effect of the chemical union of the water with the anhydrous oxalate to form a hydrous oxalate, which, being presented to the action of the water in the cell on the instant of its formation, is dissolved in a larger quantity than can be retained in solution, and thus whilst one portion of hydrous oxalate is being formed and dissolved, that which was formed previously is in the act of being deposited, and thus these processes go on simultaneously until all the amorphous oxalate which was introduced into the cell is changed into well-defined crystals, as was before explained in treating of the compound salt of the oxalate and chloride of strontium.

In conclusion I may add that the oxalates are not the only compounds of the alkaline earths which form double salts when brought into contact with strong solutions of chlorides of the same base. Crystals of tartrate of lime put into a saturated solution of chloride of calcium, form a double salt of tartrate and chloride of calcium, which is decomposed by water into tartrate of lime and chloride of calcium. Also the carbonate of lime, under favourable circumstances, will combine with the chloride of calcium to form a double salt which is acted upon by water, in the same manner as those above treated of. To obtain this last-named compound, it is necessary to put crystals of carbonate of soda or potash into a relatively large quantity of a saturated solution of chloride of calcium, so that the carbonate of lime, immediately on its coming into existence, may be acted upon by a solution of the requisite strength. Solid particles of carbonate of lime, whether crystalline or globular, are not affected by a saturated solution of chloride of calcium. Without doubt these experiments might be so modified and extended as to embrace other substances which are of very different degrees of solubility, and which still do not decompose one another; but as there is no apparent limit to such a course of experiments, I must now conclude my communication, hoping that the (in some respects)

novel method of chemical research which I have here exemplified may be considered of sufficient interest to be followed out by other investigators, and believing that the experiments and suggestions which I have here given, and the principles they involve, are calculated to throw some light on the nature of those chemical processes which take place in nature, whether in organized bodies or in the crust of the earth, neither of which branches of natural chemistry is at present sufficiently understood, and both of which it is of very high interest further to elucidate.

April 6, 1865.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "Report on the New Unit of Electrical Resistance proposed and issued by the Committee on Electrical Standards appointed in 1861 by the British Association." By FLEEMING JENKIN, Esq. Communicated by Professor A. W. WILLIAMSON. Received March 20, 1865.

Sir Humphry Davy, in 1821*, published his researches proving a difference in the conducting-power of metals and the decrease of that power as their temperature rose. This quality of metals was examined by Snow Harris, Cumming, and E. Becquerel, whose table of conducting-powers, compiled by the aid of his differential galvanometer, and published in 1826†, is still frequently quoted, and is indeed remarkable as the result of experiments made before the publication by Ohm, in 1827‡, of the true mathematical theory of the galvanic circuit.

The idea of resistance as the property of a conductor was introduced by Ohm, who conceived the force of the battery overcoming the resistance of the conductors and producing the current as a result. Sir Humphry Davy, on the contrary, and other writers of his time, conceived the voltaic battery rather as continually reproducing a charge, somewhat analogous to that of a Leyden jar, which was discharged so soon as a conductor allowed the fluid to pass. The idea of resistance is the necessary corollary of the conception of a force doing some kind of work§, whereas the idea of conducting-power is the result of an obvious analogy when electricity is conceived as a fluid, or two fluids, allowed to pass in different quantities through different wires from pole to pole. When submitted to measurement, the

* Phil. Trans. 1821, vol. cxi. p. 425.

† Ann. de Chim. et de Phys. vol. xxxii. 2nd series, p. 420.

‡ Die galvanische Kette, mathematisch bearbeitet, 1827; also Taylor's Scientific Memoirs, vol. ii. p. 401.

§ The writer does not mean by this that electrical and mechanical resistance are truly analogous, or that a current truly represents work.

qualities of conducting-power and resistance are naturally expressed by reciprocal numbers, and the terms are used in this sense in the early writings of Lenz (1833) *, who, with Fechner †, and Pouillet ‡, established the truth of Ohm's theory shortly after the year 1830.

The conception of a unit of resistance is implicitly contained in the very expression of Ohm's law; but the earlier writers seem to have contented themselves with reducing by calculation the resistance of all parts of a heterogeneous circuit into a given length of some given part of that circuit, so as to form an imaginary homogeneous conductor, the idea of which lies at the basis of Ohm's reasoning. These writers, therefore, generally speak of the resistance as the "reduced length" of the conductor, a term still much used in France (*vide* Daguin, Jamin, Becquerel, De la Rive, and others). The next step would naturally be, when comparing different circuits, to reduce all resistances into a length of some one standard wire, though this wire might not form part of all or of any of the circuits, and then to treat the unit length of that standard wire as a unit of resistance. Accordingly we find Lenz (in 1838§) stating that 1 foot of No. 11 copper wire is his unit of resistance, and that it is 19·9 times as great as the unit he used in 1833*, which was a certain constant part of the old circuit. In the earlier paper the resistances are treated as lengths, in the later as so many "units."

Lenz appears to have chosen his unit at random, and apparently without the wish to impose that unit upon others. A further advance is seen when Professor Wheatstone, in his well-known paper of 1843‖, proposes 1 foot of copper wire, weighing 100 grains, not only as a unit, but as a standard of resistance, chosen with reference to the standard weight and length used in this country. To Professor Wheatstone also appears due the credit of constructing (in 1840) the first instruments by which definite multiples of the resistance-unit chosen might be added or subtracted at will from the circuit‖. He was closely followed by Poggendorff¶ and Jacobi**, the description of whose apparatus, indeed, precedes that of the Rheostat and Resistance-coils, although the writer understands that they acknowledge having cognizance of those inventions. Resistance-coils, as the means of adding, not given lengths, but given graduated resistances to any circuit, are now as necessary to the electrician as the balance to the chemist.

In 1846 Hankel†† used as unit of resistance a certain iron wire; in 1847 I. B. Cooke‡‡ speaks of a length of wire of such section and conducting-power as is best fitted for a standard of resistance. Buff §§ and Horsford ‖‖

* Pogg. Ann. vol. xxxiv. p. 418.

† Maasbestimmungen, etc. 1 vol. 4to. Leipzic, 1831.

‡ Elémens de Physique, p. 210, 5th edition; and Comptes Rendus, vol. iv. p. 267.

§ Pogg. Ann. vol. xlv. p. 105.

‖ Phil. Trans. 1843, vol. cxxxiii. p. 303.

¶ Pogg. Ann. vol. lii. p. 511.

** Pogg. Ann. vol. lii. p. 526; vol. liv. p. 347.

†† Pogg. Ann. vol. lxix. p. 255.

‡‡ Phil. Mag. New Series, vol. xxx. p. 385.

§§ Pogg. Ann. vol. lxxiii. p. 497.

‖‖ Pogg. Ann. vol. lxx. p. 238, and Silliman's Journ. vol. v. p. 36.

N 2

in the same year reduce the resistance of their experiments to lengths of a given German-silver wire, and as a further definition they give its value as compared with pure silver. To avoid the growing inconvenience of this multiplicity of standards, Jacobi * (in 1848) sent to Poggendorff and others a certain copper wire, since well known as Jacobi's standard, desiring that they would take copies of it, so that all their results might be expressed in one measure. He pointed out, with great justice, that mere definition of the standard used, as a given length and weight of wire, was insufficient, and that good copies of a standard, even if chosen at random, would be preferable to the reproduction in one laboratory of a standard prepared and kept in another. The present Committee fully indorse this view, although the definition of standards based on weights and dimensions of given materials has since then gained greatly in precision.

Until about the year 1850 measurements of resistance were confined, with few exceptions, to the laboratory; but about that time underground telegraphic wires were introduced, and were shortly followed by submarine cables, in the examination and manufacture of which the practical engineer soon found the benefit of a knowledge of electrical laws. Thus in 1847 the officers of the Electric and International Telegraph Company used resistance-coils made by Mr. W. F. Cooke, apparently multiples of Wheatstone's original standard, which was nearly equal to the No. 16 wire of commerce; and Mr. C. F. Varley † states that, even at that date, he used a rough mode of "distance testing." In 1850, Lieut. Werner Siemens ‡ published two methods for determining, by experiments made at distant stations, the position of "a fault"—that is to say, a connexion between the earth and the conducting-wire of the line at some point between the stations. In one of these plans a resistance equal to that of the battery is used, and the addition of resistances is also suggested; and Sir Charles Bright, in a Patent dated 1852 §, gives an account of a plan for determining the position of a fault by the direct use of resistance-coils. Since that time new methods of testing for faults and of examining the quality of materials employed, and the condition of the line, have been continually invented, almost all turning, more or less, on the measurement of resistance; greater accuracy has been continually demanded in the adjustment of coils and other testing-apparatus, until we have now reached a point where we look back with surprise at the rough and ready means by which the great discoveries were made on which all our work is founded.

The first effect of the commercial use of resistance was to turn the "feet" of the laboratory into "miles" of telegraph wire. Thus we find employed as units, in England the mile of No. 16 copper wire ‖, in Germany the German mile of No. 8 iron wire, and in France the kilometre of iron wire

* Comptes Rendus, 1851, vol. xxxiii. p. 277. † Letter to writer, 1865.

‡ Pogg. Ann. vol. lxxix. p. 481. § Patent No. 14,331, dated Oct. 21, 1852.

‖ A size much used in underground conductors, and equal in resistance to about double the length of the common No. 8 iron wire employed in aërial lines.

of 4 millimetres diameter. Several other units were from time to time proposed by Langsdorf *, Jacobi †, Marié-Davy ‡, Weber §, W. Thomson ||, and others, with a gradually increasing perception of the points of chief importance in a standard; but none of these were generally accepted as the one recognized measure in any country. To remedy the continually increasing evils arising from the discrepancies invariably found between different sets of coils, Dr. Werner Siemens (in 1860 ¶) constructed standards, taking as unit the resistance of a column of chemically pure mercury 1 metre long, having a section equal to 1 millimetre square, and maintained at the temperature of 0° Centigrade **. Dr. Siemens supposed that this standard could be reproduced without much difficulty where copies could not be directly obtained. Mercury had been proposed before as a fitting material for a standard by Marié-Davy and De la Rive; but Dr. Siemens merits especial recognition, as the coils and apparatus he issued have been made with great care, and have materially helped in introducing strict accuracy ††.

The question had reached this point when (in 1861) the British Association, at the suggestion of Professor W. Thomson, appointed a Committee to determine the best standard of electrical resistance. This Committee, aided by a grant from the Royal Society, has now issued a new standard, the subject of the present paper.

The writer has hitherto described those units only which are founded on a more or less arbitrary size and weight of some more or less suitable material; but measurements of resistance can be conceived and carried out entirely without reference to the special qualities of any material whatever. In 1849 Kirchhoff ‡‡ had already effected a measurement of this kind; but it is to W. Weber §§ that we owe the first distinct proposal (in 1851) of a definite system of electrical measurements, according to which resistance would be measured in terms of an absolute velocity. This system of measures he called absolute electromagnetic measure, in analogy with Gauss's nomenclature of absolute magnetic measure. The Committee have decided that Weber's proposal is far preferable to the use of any unit of the kind previously described. Setting aside the difficulties in the way of their

* Liebig's Ann. vol. lxxxv. p. 155.

† Pogg. Ann. vol. lxxviii. p. 173.

‡ Ann. Chim. et Phys. 3rd series, vol. ix. p. 410.

§ Pogg. Ann. vol. lxxxii. p. 337.

|| Phil. Mag. Dec. 1851, 4th ser. vol. ii. p. 551.

¶ Pogg. Ann. vol. cx. p. 1.

** Dr. Siemens, while retaining his definition, has altered the value of his standard about 2 per cent. since the first issue; and it is doubtful whether even the present standard represents the definition truly: his experiments were made by weight; and in reducing the results to simple measurements of length he has used a specific gravity for mercury of 13·557 instead of 13·596 as given by Regnault, 13·595 by H. Kopp, and 13·594 by Balfour Stewart.

†† Many of the different units described above were represented by resistance-coils in the International Exhibition of 1862: *vide* Jury Report, Class XIII. p. 83, where their relative values are given: *vide* also Appendix A. to present paper.

‡‡ Pogg. Ann. vol. lxxvi. p. 412.

§§ Ibid. vol. lxxxii. p. 337.

reproduction, which are by no means contemptible, arbitrary material standards, whether of mercury, gold, silver, platinum, or any other material, would be heterogeneous isolated units without any natural connexion with any other physical units. The unit proposed by Weber, on the other hand, forms part of a symmetrical natural system, including both the fundamental units of length, time, and mass, and the derived electrical units of current quantity and electromotive force. Moreover it has been shown by Professor W. Thomson*, who accepted and extended Weber's proposal immediately on its appearance, that the unit of absolute work, the connecting link between all physical forces, forms part of the same system, and may be used as the basis of the definition of the absolute electromagnetic units.

The full grounds of the choice of the Committee could only be explained by a needless repetition of the arguments given in the reports already made to the British Association. It will be sufficient here to state that, in the absolute electromagnetic system, the following equations exist between the mechanical and electrical units:—

$$W=C^2Rt, \quad \ldots \ldots \ldots \ldots \quad (1)$$

where W is the work done in the time t by the current C conveyed through a conductor of the resistance R. This equation expresses Joule and Thomson's law.

$$C=\frac{E}{R}, \quad \ldots \ldots \ldots \ldots \quad (2)$$

where E is the electromotive force. This equation expresses Ohm's law.

$$Q=Ct, \quad \ldots \ldots \ldots \ldots \quad (3)$$

expressing a relation first proved by Faraday, where Q is the quantity of electricity conveyed or neutralized by the current in the time t. Finally, the whole system is rendered determinate by the condition that the unit length of the unit current must produce the unit force on the unit pole (Gauss) at the unit distance. If it is preferred to omit the conception of magnetism, this last statement is exactly equivalent to saying that the unit current conducted round two circles of unit area in vertical planes at right angles to each other, one circuit being at a great distance D above the other, will cause a couple to act between the circuits of a magnitude equal to the reciprocal of the cube of the distance D. This last relation expresses the proposal made by Weber for connecting the electric and magnetic measure. These four relations serve to define the four magnitudes R, C, Q, and E, without reference to any but the fundamental units of time, space, and mass; and when reduced to these fundamental units, it will be found that the measurement of R involves simply a velocity, *i. e.* the quotient of a length by a time. It is for this reason that the absolute

* Phil. Mag. Dec. 1851, 4th series, vol. ii. p. 551.

measure of resistance is styled $\frac{\text{metre}}{\text{second}}$ or $\frac{\text{foot}}{\text{second}}$, precisely as the common non-absolute unit of work involving the product of a weight into a length is styled kilogrammetre or foot-pound. The Committee have chosen as fundamental units the second of time, the metre, and the mass of the Paris gramme. The metrical rather than the British system of units was selected, in the hope that the new unit might so find better acceptance abroad, and with the feeling that while there is a possibility that we may accept foreign measures, there is no chance that the Continent will adopt ours. The unit of force is taken as the force capable of producing in one second a velocity of one metre per second in the mass of a Paris gramme, and the unit of work as that which would be done by the above force acting through one metre of space. These points are very fully explained in the British Association Report for 1863, and in the Appendix C to that Report by Professor J. Clerk Maxwell and the writer.

The magnitude of the $\frac{\text{metre}}{\text{second}}$ is far too small to be practically convenient, and the Committee have therefore, while adopting the system, chosen as their standard a decimal multiple 10^{10} times as great as Weber's unit $\left(\text{the } \frac{\text{millimetre}}{\text{second}}\right)$, or 10^7 times as great as the $\frac{\text{metre}}{\text{second}}$. This magnitude is not very different from Siemens's mercury unit, which has been found convenient in practice. It is about the twenty-fifth part of the mile of No. 16 impure copper wire used as a standard by the Electric and International Company, and about once and a half Jacobi's unit*.

It was found necessary to undertake entirely fresh experiments in order to determine the actual value of the abstract standard, and to express the same in a material standard which might form the basis of sets of resistance-coils to be used in the usual manner. These experiments, made during two years with two distinct sets of apparatus by Professor J. C. Maxwell and the writer, according to a plan devised by Professor W. Thomson, are fully described in the Reports to the British Association for 1863 and 1864.

The results of the two series of experiments made in the two years agree within 0·2 per cent., and they show that the new standard does not probably differ from true absolute measure by 0·1 per cent†. It is not far from the mean of a somewhat widely differing series of determinations by Weber.

In order to avoid the inconvenience of a fluctuating standard, it is proposed that the new standard shall not be called "absolute measure," or described as so many $\frac{\text{metre}}{\text{seconds}}$, but that it shall receive a distinctive name, such as the B. A. unit, or, as Mr. Latimer Clark suggests, the "Ohmad," so

* This last number may be 30 per cent. wrong, as the writer has never been in possession of an authenticated Jacobi standard, and has only arrived at a rough idea of its value by a series of published values which afford an indirect comparison.

† *Vide* Appendix B.

that, if hereafter improved methods of determination in absolute measure are discovered or better experiments made, the standard need not be changed, but a small coefficient of correction applied in those cases in which it is necessary to convert the B. A. measure into absolute measure. Every unit in popular use has a distinctive name; we say feet or grains, not units of length or units of weight; and it is in this way only that ambiguity can be avoided. There are many absolute measures, according as the foot and grain, the millimetre and milligramme, the metre and gramme, &c. are used as the basis of the system. Another chance of error arises from the possibility of a mistake in the decimal multiple used as standard. For all these reasons, as well as for convenience of expression, the writer would be glad if Mr. Clark's proposal were adopted and the unit called an Ohmad.

Experiments have been made for the Committee by Dr. Matthiessen, to determine how far the permanency of material standards may be relied on, and under what conditions wires unaltered in dimension, in chemical composition, or in temperature change their resistance. Dr. Matthiessen has established that in some metals a partial annealing, diminishing their resistance, does take place, apparently due to age only. Other metals exhibit no alteration of this kind; and no permanent change due to the passage of voltaic currents has been detected in any wires of any metal—a conclusion contrary to a belief which has very generally prevailed.

The standard obtained has been expressed in platinum, in a gold-silver alloy, in a platinum-silver alloy, in a platinum-iridium alloy, and in mercury. Two equal standards have been prepared in each metal; so that should time or accident cause a change in one or more, this change will be detected by reference to the others. The experiments and considerations which have led to the choice of the above materials are fully given in the Report to the British Association for 1864. The standards of solid metals are wires of from 0·5 millim. to 0·8 millim. diameter, and varying from one to two metres in length, insulated with white silk wound round a long hollow bobbin, and then saturated with solid paraffin. The long hollow form chosen allows the coils rapidly to assume the temperature of any surrounding medium, and they can be plunged, without injury, into a bath of water at the temperature at which they correctly express the standard. The mercury standards consist of two glass tubes about three-quarters of a metre in length. All these standards are equal to one another at some temperature stated on each coil, and lying between 14°·5 and 16°·5 C. None of them, when correct, differ more than 0·03 per cent. from their value at 15°·5 C.

Serious errors have occasionally been introduced into observations by resistance at connexions between different parts of a voltaic circuit, as perfect metallic contact at these points is often prevented by oxide or dirt of some kind. Professor Thomson's method of inserting resistances in the Wheatstone balance (differential measurer) has been adopted for the standards, but

in the use of the copies which have been issued it has been thought that sufficient accuracy would be attained by the use of amalgamated mercury connexions.

In the standards themselves permanence is the one paramount quality to be aimed at; but in copies for practical use a material which changes little in resistance with change of temperature is very desirable, as otherwise much time is lost in waiting till coils have cooled after the passage of a current; moreover large corrections have otherwise to be employed when the coils are used at various temperatures; and these temperatures are frequently not known with perfect accuracy. German silver, a suitable material in this respect, and much used hitherto, has been found to alter in resistance, in some cases, without any known cause but the lapse of time, since the change has been observed where the wires were carefully protected against mechanical or chemical injury. A platinum-silver alloy has been preferred by the Committee to German silver for the copies which have been made of the standard. These have been adjusted by Dr. Matthiessen so as to be correct at some temperature not differing more than 1° from 15°·5 C. The resistance of platinum-silver changes about 0·031 per cent. for each degree Centigrade within the limits of 5° above and below this temperature; this change is even less than that of German silver. The new material seems also likely to be very permanent, as it is little affected by annealing. The form of the copies is the same as that of the standard, with the exception of the terminals, which are simple copper rods ending in an amalgamated surface. Twenty copies have been distributed gratis, and notices issued that others can be procured from the Committee for £2 10*s*. The Committee also propose to verify, at a small charge, any coils made by opticians, as is done for thermometers and barometers at Kew.

Dr. Matthiessen reports, with reference to the question of reproduction, that given weights and dimensions of several pure metals might be employed for this purpose *if absolute care were taken.* The reproduction, in this manner, of the mercury unit, as defined by Dr. Siemens, differs from the standards issued by him in 1864 about 8·2 per thousand if the same specific gravity of mercury be used for both observations*. Each observer uses for his final value the mean of several extremely accordant results. It is therefore to be hoped that the standard will never have to be reproduced by this or any similar method. On the other hand, four distinct observers, with four different apparatus, using four different pairs of standards issued respectively by Dr. Siemens and the Committee, give the B. A. unit as respectively equal to 1·0456, 1·0455, 1·0456, and 1·0457 of Siemens's 1864 unit. It is certain that two resistances can be compared with an accuracy of one part in one hundred thousand—an accuracy wholly unattainable in any reproduction by weights and measures of a given body, or by fresh reference to experiments on the absolute resistance. The above four com-

* If Dr. Matthiessen uses the sp. gr. of 13·596, as given by Regnault, the difference from Dr. Siemens's standard is 5 per thousand.

parisons, two of which were made by practical engineers, show how far the present practice and requirements differ from those of twenty and even ten years ago, when, although the change of resistance due to change of temperature was known, it was not thought necessary to specify the temperature at which the copper or silver standard used was correct. The difficulty of reproducing a standard by simple reference to a pure metal, further shows the unsatisfactory nature of that system in which the conducting-power of substances is measured by comparison with that of some other body, such as silver or mercury. Dr. Matthiessen has frequently pointed out the discrepancies thus produced, although he has himself followed the same system pending the final selection of a unit of resistance. It is hoped that for the future this quality of materials will always be expressed as a specific resistance or specific conducting-power referred to the unit of mass or the unit of volume, and measured in terms of the standard unit resistance, that the words conducting-power will invariably be used to signify the reciprocal of resistance, and that the vague terms good and bad conductor or insulator will be replaced, in all writings aiming at scientific accuracy, by those exact measurements which can now be made with far greater ease than equally accurate measurements of length.

There is every reason to believe that the new standard will be gladly accepted throughout Great Britain and the colonies. Indeed the only obstacle to its introduction arises from the difficulty of explaining to inquirers what the unit is. The writer has been so much perplexed by this simple question, finding himself unable to answer it without entering at large on the subject of electrical measurement, that he has been led to devise the following definitions, in which none but already established measures are referred to.

The resistance of the absolute $\frac{\text{metre}}{\text{second}}$ is such that the current generated in a circuit of that resistance by the electromotive force due to a straight bar 1 metre long moving across a magnetic field of unit intensity* perpendicularly to the lines of force and to its own direction with a velocity of 1 metre per second, would, if doing no other work or equivalent of work, develope in that circuit in one second of time a total amount of heat equivalent to one absolute unit of work—or sufficient heat, according to Dr. Joule's experiments, to heat 0·0002405 gramme of water at its maximum density 1° Centigrade.

The new standard issued is as close an approximation as could be obtained by the Committee to a resistance ten million times as great as the absolute $\frac{\text{metre}}{\text{second}}$. The straight bar moving as described above in a magnetic field of unit intensity, would require to move with a velocity of ten millions of metres per second to produce an electromotive force which would generate in a circuit of the resistance of the new standard the same

* Gauss's definition.

current as would be produced in the circuit of one $\frac{\text{metre}}{\text{second}}$ resistance by the electromotive force due to the motion of the bar at a velocity of one metre per second. The velocity required to produce this particular current* being in each case proportional to the resistance of the circuit, may be used to measure that resistance, and the resistance of the B. A. unit may therefore be said to be ten millions of metres per second, or $10^7 \frac{\text{metres}}{\text{second}}$.

It is feared that these statements are still too complex to fulfil the purpose of popular definitions, but they may serve at least to show how a real velocity may be used to measure a resistance by using the velocity with which, under certain circumstances, part of a circuit must be made to move in order to induce a given current in a circuit of the resistance to be measured. That current in the absolute system is the unit current, and the work done by that unit current in the unit of time is equal to the resistance of the circuit, as results from the first equation stated above.

Those who from this slight sketch may desire to know more of the subject will find full information in the Reports of the Committee to the British Association in 1862, 1863, and 1864. The Committee continue to act with the view of establishing and issuing the correlative units of current, electromotive force, quantity, and capacity, the standard apparatus for which will, it is proposed, be deposited at Kew along with the ten standards of resistance already constructed with the funds voted by the Royal Society.

Appendix B.

The following Table shows the degree of concordance obtained in the separate experiments used to determine the unit. The determinations were made by observing the deflections of a certain magnet when a coil revolved at a given speed, first in one direction, and then in the opposite direction. The first column shows the speed in each experiment; the second shows the value of the B. A. unit in terms of $10^7 \frac{\text{metres}}{\text{second}}$, as calculated from the single experiments. A difference constantly in one direction may be observed in the values obtained when the coil revolved different ways. This difference depended on a slight bias of the suspending thread in one direction. The third column shows the value of the B. A. unit calculated from the pair of experiments. The fourth shows the error of the pair from the mean value finally adopted. In the final mean adopted, the 1864 determination was allowed five times the weight allowed to that of 1863.

* This current is the unit current, and, if doing no other work or equivalent of work, would develope, in a circuit of the resistance of the B. A unit, heat equivalent to ten millions of units of work, or enough to raise the temperature of 2405 grammes of water at its maximum density 1° Centigrade.

1. Time of 100 revolutions of coil, in seconds.	2. Value of B. A. unit in terms of $10^7 \frac{\text{metres}}{\text{second}}$, as calculated from each experiment.	3. Value from mean of each pair of experiments.	4. Percentage error of pair of observations from mean value.
17·54 17·58	1·0121 0·9836	0·9978	−0·22
77·62 76·17	1·0468 0·9613	1·0040	+0·40
53·97 54·53	0·9985 0·9998	0·9992	−0·08
41·76 41·79	0·9915 0·9936	0·9925	−0·75
54·07 53·78	0·9961 0·9886	0·9924	−0·76
17·697 17·783	0·9878 1·0136	1·0007	+0·07
17·81 17·78	0·9952 1·0174	1·0063	+0·63
17·01 16·89	1·0191 0·9895	1·0043	+0·43
21·35 21·38	1·0034 1·0011	1·0022	+0·22
21·362 21·643	0·9968 1·0096	1·0040	+0·40
11·247 16·737	1·0424 0·9707	0·9981	−0·19

Probable error of R (1864).......... =0·1 per cent.
Probable error of R (1863).......... =0·24 „
Difference in two values 1864 and 1863=0·16 „
Probable error of two experiments .... =0·08 „

II. "Researches on the Hydrocarbons of the Series $C_n\ H_{2n+2}$." By C. Schorlemmer, Esq., Assistant in the Laboratory of Owens College, Manchester. Communicated by Prof. H. E. Roscoe, F.R.S. Received March 21, 1865.

Previously to the year 1848 none of the members of the numerous family of hydrocarbons of the general formula $C_n\ H_{2n+2}$, with the single exception of marsh-gas, were known to the chemist; but since that year the researches of Kolbe on the electrolysis of the fatty acids, and those of Frankland on the action of zinc upon the alcohol iodides, have opened up a new field of discovery, from which in rapid succession rich harvests have been reaped. The hydrocarbons thus isolated were considered by their discoverers to be the true radicals of the alcohols; and in consequence the molecular weights which were then given to these bodies amounted only to half those which are now generally accepted.

In their Report to the Paris Academy upon these hydrocarbons, Laurent and Gerhardt proposed that their formulæ should be doubled, because these bodies, if represented by the smaller formulæ, would in the gaseous state occupy two volumes instead of the four volumes in which the molecule of all other organic compounds was found to occur; and they considered these bodies as homologues of marsh-gas. Hofmann afterwards expressed himself in favour of the larger formulæ on the same grounds, and also because, if Frankland's and Kolbe's formulæ are accepted, the increase in the boiling-point produced by an increase of CH_2 in the hydrocarbon would be double that which has been observed as the difference in the boiling-points of other homologous series.

Besides these radicals, Frankland discovered another series of hydrocarbons, which, according to the mode of their formation, he regarded as the hydrides of the radicals, and as the true homologues of marsh-gas, and which, according to a view first propounded by Brodie, are considered to stand in a similar relation to the radical hydrocarbons as alcohol stands to ether, viz.—

Hydride of ethyl.	Ethyl.
$\left.\begin{matrix} C_2 H_5 \\ H \end{matrix}\right\}$	$\left.\begin{matrix} C_2 H_5 \\ C_2 H_5 \end{matrix}\right\}$
Alcohol.	Ether.
$\left.\begin{matrix} C_2 H_5 \\ H \end{matrix}\right\} O.$	$\left.\begin{matrix} C_2 H_5 \\ C_2 H_5 \end{matrix}\right\} O$

Brodie anticipated the existence of mixed radicals, as ethyl-amyl, $\left.\begin{matrix} C_2 H_5 \\ C_5 H_{11} \end{matrix}\right\}$, bearing the same relation to the simple radical, $\left.\begin{matrix} C_2 H_5 \\ C_2 H_5 \end{matrix}\right\}$, as Williamson's mixed ether, $\left.\begin{matrix} C_2 H_5 \\ C_5 H_{11} \end{matrix}\right\} O$, to common ether, $\left.\begin{matrix} C_2 H_5 \\ C_2 H_5 \end{matrix}\right\} O$. The researches of Wurtz have fully realized this anticipation. Wurtz discovered a new method of preparing the alcohol radicals by the action of sodium upon the iodides; and according to this method he not only obtained the hydrocarbons discovered by Kolbe and Frankland, but, by employing two different iodides, he prepared a number of mixed radicals, which he also obtained by the electrolysis of a mixture of two fatty acids.

The results of Wurtz's investigation have always been regarded as a convincing proof of the correctness of Brodie's view, and it is now generally believed that two series of hydrocarbons of the formula $C_n H_{2n+2}$ exist, the hydrides and the radicals, the molecule of the latter containing two atoms of the real radicals which are supposed to exist in the alcohols.

A very remarkable resemblance is observed in the general physical properties of the two series, the members of which are gases or liquids so indifferent as to resist even the action of concentrated sulphuric or nitric acids. This resemblance is so great that Greville Williams was led to the opinion that the indifferent hydrocarbons which he discovered in the oils obtained in the destructive distillation of Boghead coal belonged to the

series of the radicals, although he observed some differences in their physical properties.

The chemical behaviour of the radicals has been very imperfectly studied, all experiments having failed which were carried out with the view of obtaining from a radical either the alcohol from which it was derived, or the corresponding acid; whilst, by the action of chlorine, only substitution-products had been formed, in which 2 or 4 atoms of hydrogen are replaced by chlorine.

The action of chlorine upon the hydrides had also been studied. Dumas showed that by acting upon marsh-gas, as first substitution-product the compound $CH_3\,Cl$ was formed. Berthelot proved that this body was chloride of methyl, by converting it into methyl-alcohol and other methyl-compounds.

From hydride of ethyl Frankland and Kolbe obtained the compound $C_2\,H_5\,Cl$, which, however, appeared to them not to be identical, but only isomeric with chloride of ethyl. During the last few years, however, our knowledge of the hydrides has become much more complete. Pelouze and Cahours discovered the whole series, from hydride of butyl upwards, in the American petroleum; Greville Williams proved the existence of hydride of amyl in the oils from Boghead coal, from which he inferred that the hydrocarbons formerly described as radicals were really hydrides; and I found the same hydrocarbons in the products of the destructive distillation of Cannel-coal. From these researches it appears that the reaction by which Berthelot had obtained methyl-compounds from marsh-gas is a general one, and that from each hydride the corresponding chloride, the alcohol, and all their derivatives can be prepared.

Whilst pursuing the investigation of the hydrides, I was struck by their close resemblance to the isomeric terms of the radical series, and I thought it might be possible that the opinion held by Laurent and Gerhardt was, after all, the correct one, and that the so-called radicals belonged really to the marsh-gas hydrocarbons.

Moreover Wurtz, by acting on zinc-ethyl with iodide of allyl, had obtained the mixed radical ethyl-allyl, $\left.\begin{matrix} C_2\,H_5 \\ C_3\,H_5 \end{matrix}\right\} = C_5\,H_{10}$, which has not only the composition, but all the characteristic properties of amylene, and Beilstein and Rieth had effected the synthesis of propylene and amylene by the action of zinc-ethyl upon chloroform.

Might not the synthesis of the alcohol radicals be a synthesis of the same kind? Or, if hydrides and radicals are really different, what is this difference? In order to solve this question I endeavoured to try if I could replace in a radical hydrocarbon 1 atom of hydrogen by chlorine, in order to compare these products and their derivatives with the chlorides and other derivatives obtained from the hydrides.

As a starting-point I selected the mixed radical ethyl-amyl, because this hydrocarbon may easily be obtained in sufficient quantity, and because I

had previously carefully studied the hydride of heptyl and its derivatives. In the preparation of ethyl-amyl a considerable quantity of the radical amyl is always formed, the behaviour of which with chlorine I also investigated. The first results of these researches have been published in the 'Journal of the Chemical Society,' vol. i. (new ser.) p. 425. I obtained the chlorides $C_7 H_{15} Cl$ and $C_{10} H_{21} Cl$, which appeared to be identical with the chlorides of heptyl and of decatyl. The next step was to ascertain how the lower terms of the two series are acted upon by chlorine, and to study closely the differences which were stated to exist between methyl and hydride of ethyl. The results which I obtained were, however, quite different from those of former observers. I found that when equal volumes of chlorine and of methyl, and equal volumes of chlorine and hydride of ethyl, are exposed to the diffused daylight, the principal product of the reaction consists in both cases of the compound $C_2 H_5 Cl$, a body having the composition and characteristic properties of chloride of ethyl, and as neither in the physical nor in the chemical properties of the two hydrocarbons a difference is known to exist, I concluded that methyl and hydride of ethyl are identical. It appeared very probable that the same relation might exist in the case of the higher terms of the two series, which, however, showed some differences in their physical properties, and I left it an open question whether there is only one series of hydrocarbons, $C_n H_{2n+2}$, or whether two series exist which exhibit the characters of physical isomerism.

The following communication contains the results of researches carried out for the purpose of deciding the above question in the case of the hydrocarbons ethyl-amyl and hydride of heptyl, and of amyl and hydride of decatyl. The results of this investigation are in some respect not so complete as I could desire. The chief difficulty in working upon this subject is the very small yield of the alcohol of the series which is obtained from a proportionally large quantity of the hydrocarbon, the alcohol being certainly that compound by the study of which and of its derivatives much light would be thrown on many still obscure points. Only, in the most favourable cases, one-third of the theoretical yield of the chloride is obtained; and in preparing the acetate from the chloride a large quantity of the latter is decomposed into olefine and hydrochloric acid, and this decomposition increases as the compounds become richer in carbon. Thus a small fraction of the hydrocarbon is converted into the acetate; and in order to prepare from this ether the pure anhydrous alcohol, losses are unavoidable, which diminish considerably the yield of a pure product. I have tried in different ways to obtain other compound ethers from the chloride, or to convert it into the iodide, without finding a better method than that originally employed, to which therefore I finally returned.

The specific gravities given in the following account are compared with water at 4° C., or they give the weight of one cubic centimetre in grammes; the boiling-points are provided with the necessary correction for the mercurial column above the vapour.

I. *Heptyl Compounds.*

The ethyl-amyl which is obtained by acting upon a mixture of the iodides of ethyl and amyl with sodium contains generally traces of ethyl-ether and ethyl-amyl-ether, the formation of which is easily explained by the presence of traces of moisture and amyl-alcohol, both of which can be completely excluded only with difficulty. In order to remove these ethers, I treated the crude ethyl-amyl, from which, by fractional distillation, the amyl was as much as possible separated, and which boiled between 70°–120° C., with a mixture of concentrated nitric and sulphuric acids, by which not only the ethers, but also traces of iodides, which obstinately adhere, are removed. By washing with water, drying over caustic potash, and rectification over sodium, pure ethyl-amyl was obtained as a light mobile liquid possessing a faint ethereal odour which cannot be distinguished from that of hydride of heptyl. It boils at 90°–91° C., and has the specific gravity 0·6819 at 17°·5 C.

The boiling-point of the hydride of heptyl I have formerly stated as 98° C., whilst Pelouze and Cahours give it as 92°–94° C. I have lately convinced myself that the latter observation is the more correct one. The boiling-point of this hydrocarbon becomes lowered after being repeatedly treated with a mixture of nitric and sulphuric acids, by which traces of nitro-compounds of the benzol series of hydrocarbons are removed, which obstinately adhere*.

Mr. Ch. R. Wright continued the fractional distillation of the hydride, which was very well purified in the above manner for a long time. In the beginning of these rectifications, the largest quantity of the liquid distilled between 95°–100°, whilst always a small quantity with a boiling-point above

* Pelouze and Cahours state that the American petroleum which they used did not contain hydrocarbons of the benzol series, whilst I found a not inconsiderable quantity of these compounds in the rectified petroleum from which I isolated the hydrides. As it was not impossible that this was an accidental or intentional admixture, I endeavoured to procure some genuine crude American petroleum; but I did not succeed in obtaining crude genuine Pennsylvanian, as none of it had reached the Liverpool market for months. I however got some real Canadian rock-oil, as a thick black liquid of a very unpleasant odour. I distilled it, and treated the portion boiling below 150° C. with concentrated nitric acid, which acted violently. The acid liquid was then diluted with water, and heavy liquid nitro-compounds separated, possessing the odour of bitter almonds. These were treated with tin and hydrochloric acid, and the solution thus obtained was distilled with caustic potash. The aqueous distillate, in which some drops of an oily liquid were suspended, had the odour of aniline, and gave with a solution of bleaching-powder the most distinct aniline-reaction. The beautiful rosaniline-reaction could also easily be obtained by heating one of the oily drops with bichloride of mercury. Canadian petroleum contains therefore the series of benzol hydrocarbons. In the preparation of hydride of decatyl from rectified petroleum, the portion boiling between 150°–170° was purified by nitric and sulphuric acids, and thus liquid and solid nitro-compounds obtained. The solid portion was several times recrystallized from alcohol, and the whole of the needle-shaped crystals thus obtained gave on analysis numbers very nearly agreeing with the formula of trinitro-cumol, $C_9\,H_9\,(NO_2)_3$.

100° was left behind. As soon as such a residue ceased to be observed, the distillates were collected at intervals of 3°, and thus at last by far the largest quantity was found to boil constantly between 90°–92°. Whilst, however, the boiling-point was lowered, no change in the specific gravity was observed. The hydride of heptyl boiling at 90°–92° has the specific gravity 0·7148 at 15°, whilst that boiling at 98° gave the specific gravity 0·7149 at 15°·5. In the analysis and determination of the vapour-density of the hydrocarbon boiling at 90°–92°, Mr. Wright obtained the following data:—

(1) 0·2047 substance gave 0·631 carbonic acid and 0·2935 water.

(2) 0·2114 substance gave 0·6515 carbonic acid and 0·3030 water.

	Calculated.	Found. I.	Found. II.
C_7	84	84·08	84·05
H_{16}	16	15·93	15·93
	100	100·01	99·98

(1)	Balloon with air	6·8755
	Temperature of air	10°
	Balloon with vapour	7·0135
	Temperature on sealing	162°
	Residual air	0·2 cub. centim.
	Capacity of balloon	88·9 cub. centim.
(2)	Balloon with air	8·3717
	Temperature of air	11°
	Balloon with vapour	8·5661
	Temperature on sealing	152°
	Capacity of balloon	119·3 cub. centim.
	Residual air	0

Vapour-density calculated for $C_7 H_{16}$.	Found. I.	Found. II.
3·46	3·45	3·46

If a current of chlorine is passed into these hydrocarbons in diffused daylight, the gas is completely absorbed for some time, and the liquid assumes a yellow colour; however, suddenly it becomes heated, torrents of hydrochloric acid are evolved, and the colour of the chlorine disappears, and from this point the chlorine acts quietly, and hydrochloric acid is continuously evolved. If a little iodine has been added, the action continues in the dark, but higher chlorinated products are more easily formed. If 100–200 grammes of the hydrocarbon have been employed, the current of chlorine must be interrupted after some hours, and the liquid treated with solid caustic potash to remove the absorbed hydrochloric acid. The non-attacked hydrocarbon is then separated by distillation from the chlorinated product, and the former treated repeatedly with chlorine, until all

the hydrocarbon has been acted upon. The product is then subjected to fractional distillation, in order to isolate the pure chloride $C_7 H_{15} Cl$.

The chloride of heptyl derived from ethyl-amyl boils at 146°–148°, and has the specific gravity 0·8814 at 16°·5. The chloride from the hydride has the boiling-point 148°–150°, and the specific gravity 0·9030 at 15°; the chloride from another preparation boiled at 147°–149°, and its specific gravity was found to be 0·8965 at 19°.

By heating these chlorides in sealed glass tubes with acetate of potassium and glacial acetic acid to 160°–180°, chloride of potassium separates out, and heptylene and acetate of heptyl are formed. The point at which all the chloride has been decomposed can easily be recognized as follows:—Two layers of liquid are observed in the heated tube, the lower one consisting of a concentrated solution of acetate of potassium in acetic acid, and the upper one of chloride of heptyl with some acetic acid. Where these two layers meet, a separation of chloride of potassium takes place, and the crystals thus formed fall gradually through the lower part of the tube. As soon as this separation of the salt at the junction of the two layers ceases, the operation is finished. The contents of the tube are now diluted with water, the light liquid which separates is well washed, dried over chloride of calcium, and from this liquid heptylene and acetate of heptyl are separated by fractional distillation.

The heptylene derived from ethyl-amyl, after repeated rectifications over sodium, was obtained as a colourless mobile liquid of a faint garlic-like odour, boiling at 93°–95°, and having the specific gravity 0·7060 at 12°·5. The analysis gave the following numbers:—

0·1799 substance gave 0·5640 carbonic acid and 0·2380 water.

	Calculated.		Found.
C_7	84	85·7	85·50
H_{14}	14	14·3	14·64
	98	100·0	100·14

The heptylene from hydride of heptyl, which I have previously described, boils at 95°–97°; but even after repeated distillations the boiling-point always rises to about 100° towards the end of the operation. Its specific gravity was found to be 0·7383 at 17°·5. I may here remark that all the compounds derived from the hydride which are mentioned in this paper are those formerly described (Journ. Chem. Soc. vol. i. new series, p. 216), being prepared from the hydrocarbon boiling at 98°.

The boiling-point of the liquid from which the heptylene has been separated rises quickly above 170°, and at 180° it becomes constant, when pure acetate of heptyl distils over, giving on analysis the following results:—

0·2015 substance gave 0·5055 carbonic acid and 0·2090 water.

	Calculated.		Found.
C_9	108	68·35	68·42
H_{18}	18	11·39	11·52
O_2	32	20·26	——
	158	100·00	

This ether possesses exactly the same pleasant smell of pears as that of the acetate from hydride of heptyl. The former boils at 178°–180°, and has the specific gravity 0·8707 at 16°·5, whilst the boiling-point of the latter was found as 179°–181°, and the specific gravity 0·8868 at 19°.

This ether is easily decomposed when heated with a concentrated solution of caustic potash; heptyl-alcohol is formed, which, when dried over chloride of calcium, and treated with a small piece of sodium, in order to remove the last traces of moisture, was found to boil at 163°–165°. Its odour much resembles that of the hexyl-alcohol, but also reminds one of octyl-alcohol from castor-oil. The specific gravity is 0·8291 at 16°·5, whilst that of the hydride alcohol is 0·8479 at 16°, and its boiling-point 164°–165°. The odour of the latter is very similar to the alcohol from ethyl-amyl, but less pure, as if the true odour was interfered with by that of some other substance. The alcohol from ethyl-amyl was analyzed with the following results:—

0·2435 substance gave 0·6455 carbonic acid and 0·3075 water.

	Calculated.		Found.
C_7	84	72·4	72·30
H_{16}	16	13·8	14·03
O	16	13·8	——
	116	100·0	

Both alcohols dissolve easily in concentrated sulphuric acid: the mixture becomes hot and assumes a dark colour. After standing for some hours, a small quantity of tarry matter separates out on dilution with water, the clear liquid containing a sulpho-acid in solution, together with the excess of sulphuric acid. This mixture was neutralized with carbonate of barium, the liquid filtered, and evaporated to dryness in a water-bath. The dry residue was treated with water, and a barium salt dissolved, which however I did not succeed in obtaining crystallized from either of the alcohols, as the solutions, evaporated both in the water-bath and over sulphuric acid, yielded a thick syrupy liquid, drying slowly to a gum-like mass, in which no crystals could be detected, and which readily formed with the smallest quantity of water and alcohol clear solutions, which again, by spontaneous evaporation, dried into a gum. The smallness of the quantity of alcohol from ethyl-amyl at my disposal has for the present prevented me from repeating the experiment on a larger scale, which doubtless would have given a better result.

In order to obtain the oxidation products of the two alcohols, they were distilled in a small retort with a mixture of bichromate of potassium and diluted sulphuric acid. A violent reaction occurs at first, which soon diminishes. The distillate was shaken with a solution of carbonate of sodium, and the liquid which did not dissolve treated again several times with the oxidizing mixture, and the distillate after each treatment shaken with the solution of carbonate of sodium.

The solution of the sodium salt was evaporated to dryness in the water-bath, the residue distilled with diluted sulphuric acid, and the acid distillate, consisting of an aqueous liquid on which an oily layer swam, was rectified, in order to separate traces of sulphuric acid which had spirted over. The oily acid thus obtained possesses the odour of œnanthylic acid, and consists entirely of this compound, as the following analyses of the silver-salt prove.

(*a*) The acid derived from the hydride alcohol was neutralized with ammonia, and precipitated with a solution of nitrate of silver. The white flocculent precipitate was washed with cold distilled water, and dried carefully at 100°, when it assumed a greyish colour.

(1) 0·2084 of this salt gave 0·0948 silver.

(2) 0·1600 of this salt gave 0·0731 silver.

(3) 0·1375 of the salt recrystallized from water acidulated with nitric acid gave 0·0620 silver.

(*b*) Acid from ethyl-amyl.

(4) 0·2320 of the silver salt prepared as the salts in analyses 1 and 2 gave 0·1065 silver.

(5) 0·1790 of silver salt obtained by neutralizing the acid with carbonate of silver gave 0·0816 silver.

Calculated for $C_7H_{13}AqO_2$.	Found.				
	I.	II.	III.	IV.	V.
45·57 °/₀ Aq.	45·49	45·69	45·09	45·73	45·59

In the oxidation of both of the alcohols a strong smell of œnanthol is observed. After treating the last distillate with sodium, a small quantity of an oily liquid remained undissolved, possessing the odour of œnanthol, and boiling between 150°–160°. These liquids gave a crystalline magma by shaking with a concentrated solution of bisulphite of sodium, a few drops of a liquid having a pleasant smell remaining undissolved.

II. *Decatyl Compounds.*

The amyl used in these researches was purified, in order to remove traces of amyl-ether and iodide of amyl, exactly in the same manner as ethyl-amyl. It boiled constantly at 158°–159°, and had the specific gravity 0·7275 at 14°. The hydride of decatyl was isolated from rectified American petroleum, after purifying the portion boiling between 150°–170° by a mixture of concentrated nitric and sulphuric acids. It boiled at 157°–159°, and had the specific gravity 0·7461 at 14°. Pelouze and Cahours found the boiling-point of this hydrocarbon to be 160°, and its specific gravity 0·735 at 15°. The same hydrocarbon was found by Greville Williams in the oils from Boghead coal, and described as amyl. He gives the boiling-point 159°, and the specific gravity 0·7365 at 18°.

Amyl and the decatyl-hydride cannot be distinguished by their odour, which is exactly the same in the case of all the hydrides and the radical hydrocarbons, with the only difference that it is stronger the more volatile the substances are.

In order to obtain the chlorides $C_{10} H_{21} Cl$, I proceeded in the same manner as described in the preparation of chloride of heptyl.

The chloride of decatyl from amyl is a colourless mobile liquid of a pleasant, fruity smell; it boils at 203°–205°, and has the specific gravity 0·8739 at 14°. The chloride prepared from hydride of decatyl boils at the same temperature, 203°–205°; its specific gravity is 0·898 at 16°·5. The odour of this chloride is fainter, not quite so pleasant, as if the true smell was hidden by that of some impurity. At first it possesses a yellowish colour, as Pelouze and Cahours have already observed; but it can be obtained quite colourless by repeated distillations, when a small quantity of a brown residue is always left behind.

The analysis of the chloride from amyl I have already given in the previous paper; that of the chloride from the hydride gave the following results:—

0·2857 substance gave 0·1938 chloride of silver and 0·0296 metallic silver.

Calculated for $C_{10} H_{21} Cl$.	Found.
20·11 % Cl	20·18 %

Both chlorides are decomposed in exactly the same manner, when heated with acetate of potassium and acetic acid, as described under acetate of heptyl. The principal product consists of decatylene (diamylene) $C_{10} H_{20}$, and only a small quantity of acetate of decatyl is formed. Both compounds were separated and purified like the corresponding heptyl compounds.

The decatylene from amyl has the specific gravity 0·7438 at 18°, and boils at 160°–161°; its analysis is given under (1). The decatylene derived from the hydride boils at 157°–159°, and its specific gravity is 0·7596 at 12°; the analysis of it is given under (2). Both are colourless mobile liquids with only a faint odour.

(1) 0·2332 decatylene from amyl gave 0·7340 carbonic acid and 0·3000 water.

(2) 0·2965 decatylene from the hydride gave 0·931 carbonic acid and 0·383 water.

	Calculated.		Found. I.	Found. II.
C_{10}	120	85·7	85·84	85·64
H_{20}	20	14·3	14·29	14·35
	140	100·0	100·13	99·99

The acetate of decatyl from amyl is a colourless liquid, possessing a pleasant smell of oranges; it boils at 227°–231°, and has the specific gravity 0·8711 at 16° C. The acetate derived from the hydride has a similar odour, but not quite so pleasant; it boils at 226°–230°, and has the specific gravity 0·8750 at 15°. Both compounds gave the following analytical data:—

(1) 0·3067 of the acetate from amyl gave 0·8065 carbonic acid and 0·3270 water.

(2) 0·2474 of the acetate from the hydride gave 0·6495 carbonic acid and 0·267 water.

	Calculated.		Found. I.	Found. II.
C_{12}	144	72	71·72	71·58
H_{24}	24	12	11·85	11·99
O_2	32	16	——	——
	200	100		

The alcohols $C_{10}H_{22}O$ were obtained from the acetates by heating them with a concentrated solution of caustic potash, and drying the liquids over chloride of calcium and a little sodium. The alcohol derived from amyl boils between 210°–215°; its specific gravity is 0·8257 at 12°. It possesses a pleasant penetrating odour, resembling that of the flowers of *Daphne odorata*; but also has some resemblance to the smell of octyl-alcohol.

The alcohol obtained from the hydride has a similar odour, but not quite so pleasant; it boils also at 210°–215°, and has at 14° the specific gravity 0·8380. The following data give the results of the analysis of the alcohols:—

(1) 0·1846 of the alcohol from amyl gave 0·5120 carbonic acid and 0·2305 water.

(2) 0·1834 of the alcohol from hydride gave 0·5075 carbonic acid and 0·2320 water.

	Calculated.		Found. I.	Found. II.
C_{10}	120	75·94	75·74	75·49
H_{22}	22	13·93	13·88	14·05
O	16	10·13	——	——
	158	100·00		

The alcohols and acetates were burnt with oxide of copper alone, without a current of oxygen, and this will account for the loss in carbon.

Unfortunately I obtained these alcohols in small quantity only, and I am, therefore, obliged to postpone for the present the investigation of their derivatives. Both dissolve easily in concentrated sulphuric acid; but I did not succeed in obtaining either of the sulpho-salts in the crystallized state. I have not yet studied the products of oxidation of these alcohols.

From the above researches I conclude that no difference exists in the chemical behaviour of the radicals and of the hydrides; the difference which has been observed in the physical properties, as in the boiling-points, also diminishes the better and the more closely these hydrocarbons are studied. There are, however, differences in the specific gravities which cannot be overlooked. The radicals as well as their derivatives have a lower specific gravity than the corresponding hydrocarbons from petroleum and their derivatives. It is, however, a well-established fact that it is im-

possible to isolate pure compounds from a mixture of many homologous compounds by even repeated fractional distillations, and I have convinced myself, whilst engaged for some time upon this subject, that it is much easier to obtain one of the hydrocarbons free from homologues having lower boiling-points, than to remove the last traces of higher boiling compounds which adhere obstinately. This is the case even, if, after repeated fractional distillations, a hydrocarbon is obtained which boils constantly between two degrees, and from it a chloride is prepared which also boils constantly between two degrees. The acetate which is derived from this chloride distils for the most part between two degrees, but after each rectification a small quantity of a liquid, possessing a higher boiling-point, remains behind; and if, finally, an acetate of a constant boiling-point has been obtained, the alcohol prepared from it also contains a small quantity of a substance which raises the boiling-point towards the end of the distillation. From a constantly boiling radical, on the other hand, derivatives are obtained which have a constant boiling-point, and which are colourless; whilst those from the hydrides have generally first a yellowish colour, and leave dark residues behind on distillation, and even if they are obtained colourless, after several rectifications, they generally darken again after some time, and brown flakes separate out. These observations, as well as the unpleasant odour which all the compounds derived from the hydrocarbons from petroleum possess, prove that they contain impurities which will certainly tend to raise the specific gravities. Moreover, Dale has shown that the hydrocarbons obtained from the acids of the series $C_n H_{2n-2} O_4$ also have a much lower specific gravity than the corresponding petroleum hydrocarbons.

Hence it appears highly probable that only one series of hydrocarbons of the formula $C_n H_{2n+2}$ *exists.*

Hydride of ethyl and ethyl are compounds possessing quite an analogous constitution, and they cannot be fairly compared with alcohol and ether, in which two groups $C_2 H_5$ and H, and $C_2 H_5$, $C_2 H_5$ are separated by an oxygen atom, whilst in the hydrocarbons all the carbon atoms are united together in exactly the same manner, and thus only one graphical representation of these bodies is possible*.

If, however, as the identity of the two series has not yet been strictly proved, the view should be preferred that isomeric hydrocarbons exist, we are compelled to admit also that isomeric chlorides, compound ethers, alcohols, and olefines exist, as these derivatives of the hydrocarbons show exactly the same differences as are found in the hydrocarbons themselves, and all these bodies must then have to be considered as absolutely isomeric.

The cause of the isomerism so often observed in compounds belonging to the aromatic series, as well as the cause of homology in these bodies, has been much elucidated by the beautiful researches of Fittig on the benzol hydrocarbons. He believes that similar relations to those which he has

* *Vide* "On the Theory of Isomeric Compounds." By Dr. A. Crum Brown, Trans. Roy. Soc. Edinb. vol. xxiii. part iii. p. 707.

found for the aromatic series exist between the marsh-gas hydrocarbons and the alcohol radicals*. But Fittig's isomeric hydrocarbons belong to the class of metamers having a different chemical structure †, whilst only one kind of structure can be given for the saturated hydrocarbons of the formula $C_n H_{2n+2}$.

I am still pursuing these researches, and hope soon to obtain more definite results.

III. "Introductory Memoir on Plane Stigmatics." By Alexander J. Ellis, F.R.S. Received March 23, 1865.

(Abstract.)

If from every point in a plane curve parallel straight lines be drawn cutting a given straight line in another series of points, the first set of points, which for convenience may be termed *stigmata*, will be coordinated with the second set of points, which may be termed *indices*, in the same manner as by the system of ordinates and abscissæ in ordinary Cartesian coordinate plane geometry.

Now, the writer remarked that the essence of this coordination consisted in the relation of the two sets of points to each other forming two related figures, and that the circumstances of the ordinates being parallel, and the indices all lying upon one straight line, were accidents. Moreover, he observed that these accidents were not regarded in the ordinary Cartesian equations, where there was nothing to point out that the ordinates were parallel or the abscissæ coincident lines, nor any mention made of the direction of the ordinates and abscissæ. It seemed to him that all the anomalies which occurred in analytical geometry under the name of "imaginaries," were traceable on the one hand to these restrictions in the figure, and on the other to the absence of any indication of their existence in the equations. He therefore thought that it would be possible to generalize plane coordinate geometry as the expression of the law which connects two or more plane figures, point for point, indices with stigmata. These relations would certainly include all those of ordinary geometry, and would, apparently, explain all anomalies hitherto encountered.

It was necessary, in the first place, to form a conception of such a generalized relation between indices and stigmata. Now, in the Cartesian straight line, the lines connecting any three stigmata are proportional to the lines connecting the three corresponding indices, and any pair of the first lines are in the same or opposite directions, according to the relative directions of the corresponding pair of the second lines. If the stigma figure and index figure were no longer straight lines, this could be generalized by saying that the triangle formed by three stigmata was directly

* Kekulé, "Sur la constitution des substances aromatiques," Bull. Soc. Chim., Février 1865, p. 98.

† Annal. Chemie und Pharm. pp. 133, 222.

similar to the triangle formed by the three corresponding indices. Again, in the circle referred to rectangular coordinates, the ordinate is a mean proportional between the segments of the diameter to which it is perpendicular, that is, the angle between which segments it bisects. It was easy to generalize this by supposing two lines to be drawn from the index, wherever it might lie on a plane, to the extremities of the same diameter, and the ordinate to bisect the angle between these lines, and to be a mean proportional between their lengths. Other curves were generalized in a similar manner.

It was then necessary to have a notation which should express the relations of both magnitude and direction in one symbol. The ordinary notation was found ill adapted for the purpose. The following was therefore chosen. Capital letters were used to represent geometrical points, and two capital letters to represent a geometrical line in length and direction. The operation of changing one such directed line into another, on the same plane, which the writer had already introduced under the name of *clinant* *, was represented in the fractional form, the changed line being written below and the other above, but instead of capital letters the corresponding small letters were employed, to show that we were dealing with operations and not with quantities; and when the changed line was the axis of reference itself, it was not expressed. The notation thus introduced closely simulates that employed in M. Chasles's 'Géométrie Supérieure,' but it is totally different in principle. It has the advantage of clearly showing the geometrical operation indicated by each algebraical change, and of perfectly obeying the laws of ordinary algebra, while it not only generalizes but frequently abridges the operations of analysis. By means of these clinants it became easy to express the relations between the stigma figure and index figure by equations which are of exactly the same character as the Cartesian equations, and from which the latter, with all their results, can be strictly deduced.

In the present introductory memoir the writer has confined himself to the investigations connected with the stigmatic straight line, explaining its equation and direction, the intersections of two such lines, the angles between them, and their distances from stigmatic points. These preliminary propositions being given with the requisite detail and illustrated by deducing from them the ordinary Cartesian formulæ, the rest of the memoir is occupied with the generalization of the fundamental theories necessary for the successful application of the stigmatic theory to plane geometry; such as those relating to the stigmatic triangle, an harmonic ratio of geometrical points anywhere situate on a plane and of stigmatic rays, pencils of such rays with their homography and involution, and the complete

* "On the Laws of Operation, and the Systematization of Mathematics," 'Proceedings,' May 26, 1859, vol. x. p. 89, at bottom. "On Scalar and Clinant Algebraical Coordinate Geometry," *ibid.* March 22, 1860, vol. x. p. 420. The notation in the present memoir is new.

quadrilateral. Then the nature of the change of coordination, by which a new index figure is coordinated with the same stigma figure, is explained, and bilinear and directional coordination introduced and illustrated by applying them to deduce the usual formulæ for the transformation of Cartesian coordinates from oblique to oblique, and from oblique to polar. This is followed by the most general theory of transversals cutting or intersecting upon any stigmatic curve, and by trilinear coordination. The equation to a stigmatic point is then discussed, giving rise to classes of stigmatic curves with bipunctual and tripunctual coordination. The investigations on trilinear and tripunctual coordination contain generalizations of Professor Plücker's 'Point and Line Coordinates,' by which their precise geometrical meaning, even when "imaginary," and even in more general cases than those "imaginaries" which he contemplated, becomes manifest from the very form of the equations.

Although details have been avoided as much as possible in the latter part of the memoir, the writer hopes that sufficient has been given to enable any mathematician to apply the theory with ease and safety to the generalization and linear realization of every theory on plane geometry which has hitherto been propounded. The conception is equally applicable to solid geometry, but will there require the algebra of quaternions, which, being non-commutative, establishes a well-marked line of separation between plane and solid stigmatics. The writer has not found a trace of this generalization in the works of any previous author, but the relations, when pointed out, appear too obvious to have escaped all notice hitherto. The writer believes that in any case no complete theory, such as that presented in this memoir, has been previously founded upon any similar conception.

The Society then adjourned, over the Easter Recess, to Thursday, April 27.

April 27, 1865.

Major-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Sir Henry Holland proposed, and Dr. Bence Jones seconded, His Royal Highness the Count of Paris for election and immediate ballot.

The ballot having been taken, His Royal Highness the Count of Paris was declared duly elected.

The following communications were read :—

I. "Further Experiments on the Production of Organisms in Closed Vessels." By George Child, M.D. Communicated by Professor Phillips. Received March 30, 1865.

The researches, an account of which is contained in the following paper, are in continuation of those which, through the kindness of Prof. Phillips,

I had the honour of communicating to the Royal Society in May last, and of which an abstract appeared in the 'Proceedings' for June 16, 1864. The former series of experiments did not pretend to be, in any respect, complete. Those which I am now about to describe will, I hope, be considered to be more so in regard to one main subject of the inquiry; but they also suggest further researches upon some collateral branches of it, which I hope to find time and opportunity to prosecute.

In the former series I experimented with animal substances mixed with water and enclosed in glass bulbs in atmospheres either of common air passed through red-hot tubes or of various gases, and the result at which I arrived was that where oxygen was present organisms of a low type were produced, but not so where that gas was not present. Thus, whatever the gas employed, where the substance was not boiled, the organisms appeared; but in the instances in which the substance was boiled, they appeared where oxygen or common air was used, but not where nitrogen, hydrogen, or carbonic acid was employed. One experiment only appeared to have produced a result which could not be reconciled with the rest, viz. in which some meat and water had been boiled and sealed up in an atmosphere of nitrogen. In this, some organisms were found; but so completely was this result unlike that found in the whole of the rest of the series, that I felt convinced that some error must have been made in the experiment itself.

The experiments now to be described have a narrower range than the others. With the exception of a few, which were mere repetitions of the experiments with nitrogen just referred to, and which were undertaken solely with the view of seeing whether the experiment just mentioned were correct or not, they are confined to the single object of observing whether or not organisms are found in close vessels containing vegetable matter and water sealed up in an atmosphere of common air previously passed through an efficient heating apparatus.

In these experiments I have adopted some slight modifications of the apparatus used in the former ones. That now employed consists of a porcelain tube, the central part of which is filled with roughly pounded porcelain; one end is connected with a gas-holder, and to the other the bulb is joined which contains the substance to be experimented upon. The bulb has two narrow necks or tubes, each of which is drawn out before the experiment begins, so as to be easily sealed by the lamp; one neck is connected with the porcelain tube, as already stated, by means of an india-rubber cork, and the other is bent down and inserted into a vessel containing sulphuric acid. The central part of the porcelain tube is heated by means of a furnace, and when it has attained a vivid red heat the bulb is joined on, the end of the porcelain tube which projects from the furnace being made thoroughly hot immediately before the cork is inserted, the cork itself being taken out of boiling water, and the neck of the bulb being also heated with a spirit-lamp immediately before it is inserted into the cork. A stream of air is now passed through the apparatus by means of the gas-

holder, and bubbles through the sulphuric acid at the other end. The substance in the bulb is then boiled for ten or fifteen minutes, the lamp withdrawn, and the bulb allowed to cool while the stream of air is still passing through the porcelain tube, maintained during the whole time at a vivid red heat. When the bulb is quite cool, the necks are sealed by means of a lamp. The advantage gained by means of this apparatus is that there is only one joint the perfection of which in any degree affects the success of the experiment, and of that joint it is easy to make sure. The porcelain tube also being, for a considerable part of its length, filled with small fragments of porcelain, all heated up to redness, easily ensures that every particle of air admitted to the bulb shall be thoroughly heated. A precisely similar arrangement was used for the nitrogen experiments, substituting a glass combustion-tube filled with copper-turnings for the porcelain tube, and a piece of india-rubber tubing for the india-rubber cork. The copper oxide was reduced by means of a stream of hydrogen when necessary between one experiment and the next.

A single experiment was tried on May 18, 1864, using apparatus similar to that employed in the experiments of the previous year.

Some pea-meal infused in water was boiled in a stream of heated air, allowed to cool, and then sealed and put by. I was then prevented from resuming my experiments for several weeks.

Then several experiments were made with nitrogen, for the purpose of confirming or correcting the nitrogen experiment of the previous year. Into the particulars of these I need not now enter, further than to say that seven experiments were tried with various infusions. Five of them were afterwards examined, and in no case were any organisms found, thus confirming me in the opinion already expressed upon that experiment. The series with which I am now concerned began on July 18.

VII. July 18.—Hay infused in water three hours, then filtered and boiled 12 minutes in a stream of heated air, and sealed up as above described.

VIII. July 18.—A similar experiment: boiled 10½ minutes.

IX. July 22.—Toppings, *i. e.* coarse flour infused in cold water 3 hours, filtered and boiled 10 minutes in a similar stream of air.

X. July 22.—A similar experiment: boiled also 10 minutes.

XI. July 25.—A similar experiment: boiled 12 minutes.

XII. July 25.—A similar experiment: boiled 10 minutes.

XIII. July 28.—Some sage-leaves bruised and infused in lukewarm water previously boiled. Allowed to stand 15 hours, filtered, and the clear fluid boiled 10 minutes in a stream of heated air, as in the other cases, and sealed up.

XIV. July 28.—A similar experiment: boiled 7 minutes.

XV. July 29.—A similar infusion of celery, allowed to stand 12½ hours, and treated as the last: boiled 12 minutes.

The bulb used in this last experiment was of a different form, which I

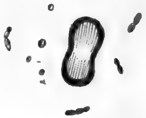
have found much more convenient, and have always employed in my subsequent experiments, which are presently to be described (as represented in the figure).

The examination of the above series of experiments took place partly on Sept. 19, when Dr. Beale kindly visited me at Oxford, in order to give me his valuable assistance, and partly at Dr. Beale's home in London, on Nov. 16, 1864.

Exp. of May 18.—Viz. pea-meal and water. In this were found small organisms moving, as given by Dr. Beale in the accompanying drawing marked Z. Their size was extremely minute, as they are here drawn as they appeared under a power of 1700.

Z.

VII.



Exp. VII.—Hay + water + heated air. Some large dumbbell-shaped crystals and a few bacteriums, very minute, but not so small as in the former case. These also are drawn by Dr. Beale.

VIII.

Exp. VIII.—The pair experiment to VII. Similar crystals, and organisms also similar, but larger. Drawn to $\frac{1}{12}$ Ross, *i. e.* 750 diameters nearly.

Exp. IX.—Coarse flour + water + heated air. The result of this experiment was unsatisfactory, and serves well to show the difficulty of the decision upon these questions.

Even with the high powers above named, we were unable to be certain of our result in this and several following cases. There were no organisms distinctly recognizable as such, but many minute round spore-like bodies moving about the field.

Exp. X.—The fellow experiment to the last, and similarly unsatisfactory.

Exp. XIII.—Sage + water + heated air. A few crystals were seen, but no organisms.

Exp. XV.—Celery + water + heated air. Some prismatic crystals; no organisms.

It was resolved to leave the rest of these experiments till a longer time should have elapsed since the vessels were closed. The examination was accordingly resumed Nov. 16.

Exp. XII.—Coarse flour + water + heated air, contained some indeterminate granular matter and some few bodies which might be dead bacteriums, but nothing that could safely be considered as such.

Exp. XI.—The fellow experiment to XII., and equally without result.

Exp. XIV.—Sage + water + heated air, gave also no definite result.

Now, omitting altogether the nitrogen experiments, seven in number, we

have here a series of ten experiments instituted with a view of showing whether organisms can be produced in vegetable infusions within closed vessels supplied with heated air. In my desire to try a variety of substances I took almost anything which my garden afforded, and in this way probably my selection of sage and celery may have been a bad one, as the aromatic ingredients of these plants may be supposed to influence the result of the experiment, especially as in a close vessel any volatile oil would be retained. If, therefore, the three experiments with these substances be eliminated, there remain seven experiments, one with pea-meal, two with hay, and four with coarse flour. Of these, five were examined on Sept. 19, and in three (viz. the pea-meal and the two hay experiments) the vessels were found to contain moving organisms. In two (those performed with coarse flour) none were found, and in the remaining two, examined on Nov. 16, also none were found.

In the meantime, when, from several of the above experiments having produced negative results, I looked upon the series as inconclusive, I instituted a fresh series of twelve experiments in the end of September, as follows.

The apparatus employed was the same as that used in the last series, except that I had some large double bulbs made for the present series. In other respects the process was the same as before.

Exp. I. Sept 30.—Hay infused $3\frac{1}{2}$ hours in water, filtered, and boiled 10 minutes in a stream of heated air—sealed up when cool.

Exp. II. Sept. 30.—Similar in all respects.

Exp. III. Oct. 1.—Similar.

Exp. IV. Oct. 1.—Similar.

Exp. V. Oct. 5.—Flour infused in warm water $3\frac{1}{2}$ hours and filtered: boiled 11 minutes, as before, and sealed.

Exp. VI. Oct. 5.—Similar: boiled 10 minutes.

Exp. VII. Oct. 5.—A similar infusion infused $6\frac{1}{2}$ hours, not filtered: boiled 10 minutes.

Exp. VIII. Oct. 5.—Similar.

Exp. IX. Oct. 7.—Flour infused $3\frac{1}{2}$ hours, not filtered: boiled 10 minutes in a stream of oxygen, and sealed as before.

Exp. X. Oct. 7.—Similar: boiled $10\frac{1}{2}$ minutes.

Exp. XI. Oct. 7.—Flour infused $4\frac{1}{2}$ hours and filtered: boiled 10 minutes in oxygen.

Exp. XII.—Similar.

On Oct. 8 this series of experiments was divided into two sets: [B], Nos. II., IV., VI., VIII., X., XII., were placed on a high shelf in my dining-room; the rest [A] in a hot closet, by the side of the cooking-stove, in the kitchen.

The object of the latter arrangement was to ensure the vessels being kept warm enough during the winter months; but the heat was, I have no doubt, too great. I saw the thermometer on more than one occa-

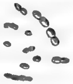
sion over 140° Fahr., and have reason to believe that I did not see it at its highest. Moreover, the bulbs here were almost wholly deprived of light. Thus, before opening the vessels, I had made up my mind that the results of the other half of the series were most to be depended upon. The temperature of the room in which they were probably never fell below 40° Fahr., and was generally between 50° and 60°.

The examination of the B division of this series took place at Dr. Beale's house, Feb. 7, 1865. The results were as follows:—

IV.

Exp. IV.—Hay + water + heated air. A few bacteriums were found in active motion (see drawing by Dr. Beale).

II.



$\frac{1}{25}$

$\frac{1}{50}$

Exp. II.—Hay + water + heated air. Very large numbers of similar organisms were found.

VI.

Exp. VI.—Flour + water + heated air. Few were found as compared with the last, but still several in active motion.

Exp. XII.—Flour + water + oxygen. No organisms found.

Exp. VIII.—Flour + water + heated air (unfiltered). A good many bacteriums, similar to the others.

Exp. X.—Flour + water + oxygen (unfiltered). Some bacteriums, but not moving.

The other set of experiments was examined by me at Oxford on various evenings between Feb. 16 and March 8; but during some part of that time I possessed no object-glass of sufficient magnifying power to avoid all uncertainty in the results.

In both of them, viz. Nos. V. and XI., I could find nothing like bacteriums. In the three others, viz. III., VII. and IX., there were what appeared to me dead ones (but a dead bacterium is an object of which few persons who have seen many would think it very safe to be very positive), and in one only, viz. No. I., an infusion of hay, were they numerous and moving. This I mention particularly, because the objects were very well seen, and moving actively in the first slide which I examined, and could be the better seen on account of the clearness of the fluid and the absence of granular matter; but upon examining several portions after the vessel had been open for a few minutes, though they continued to be seen in equally large numbers, all movement had ceased. They were examined with a $\frac{1}{25}$ object-glass of Messrs. Powell and Lealand. Now, if we omit from these two series of experiments those which I have already shown reason to distrust, we have, in all, seven in the first, and six in the second series, which seem fairly to test the question; and these having been examined by Dr. Beale, as well as myself, bacteriums were found and seen by both of us in three out of the first seven, and five out of the remaining six—in all, in eight.

Now, it may be asked, why the same or similar organisms were not found in the other cases, if the experiments were fairly tried? The answer is this, viz. that we do not know all the conditions under which they exist. It is pretty clear that they appear more easily in some substances than in others. Thus, in the first series above described, it will be noticed that the four instances in which none were found were all those in which coarse flour was the substance used. In the remaining three, where pea-meal or hay were employed, there the bacteriums were seen. So also in the other series, the one case in which nothing was found was a case in which flour was used, and in the remaining five the most numerous and distinct bacteriums were seen in the hay infusion. This may arise possibly from the fact that the infusion of flour is not so clear as the others, and always contains more granular matter; thus bacteriums are less easily distinguished in it: and, where doubtful, it is my practice to decide in the negative; that is to say, unless the bacteriums are clearly seen, I enumerate the experiment amongst those in which they are not found. Further, it is possible that in some infusions they may live and die sooner than in others, and in most of these experiments with flour there was a mass of indeterminate granular matter which might have contained the bodies of whole populations of bacteriums. Finally, it is quite possible that they might, if existing in small numbers, escape observation. Their minuteness is extreme, and observation of them far from easy. At any rate, positive evidence in a matter of this kind is of more value than negative; and the fact that in eight cases out of thirteen they have been seen, not by myself only, but also by so accurate and practised a microscopist as Dr. Beale, is of more weight than our having been unable to discover them in the remaining five cases.

The question which now remains to be discussed is, how it is that the results above given so entirely disagree with those arrived at by M. Pasteur, and now, to a certain extent, vouched for by the Commission of the Academy of Sciences. I have observed all the precautions which M. Pasteur himself speaks of as "exaggerated," yet I have shown bacteriums to be produced exactly under the circumstances in which he asserts that they do not exist. I believe this discrepancy is very easily accounted for. M. Pasteur, in his memoir, speaks of examining his substances with a power of 350 diameters. Now my experience throughout has been that it is impossible to recognize these minute objects, with any degree of certainty, even with double that magnifying power. When once their existence on a slide is shown with a power of 1500 to 1700 diameters, it is quite possible afterwards to recognize the same object with a power of 750, but I have repeatedly failed to satisfy myself in the first instance with the latter power; and on the one occasion on which I enjoyed the use of an object-glass giving a power of 3000 diameters, I found the recognition of these very minute objects rendered very much more easy. On one occasion I tried the effect of a power of 450 (not possessing one of 350), and found that all satisfactory investigation of such objects with such a power was impossible. Any

person has only to examine the drawings which accompany this communication (in one particularly, that marked Z) in order to satisfy himself that to come to any conclusion as to the presence or absence of such objects as are there represented, with a magnifying power of little more than $\frac{1}{5}$ linear measurement of that from which they are drawn, would be quite impossible. The Commission of the Academy of Sciences, which has not yet concluded its labours, has not, so far as its present report goes, concerned itself with the microscopy of the question; it has, in fact, confined itself to the dispute (which has almost become a personal one) between MM. Pasteur and Pouchet. It is worth noticing, that the fact so often referred to by writers on this subject, of the fluid in the closed vessels becoming cloudy or not as a test of the presence or absence of bacteriums, is not satisfactory; I have constantly predicted, from the cloudiness or clearness of an infusion, the presence or absence of bacteriums, and very frequently been mistaken—quite as often too in the former case as in the latter.

As to the conclusions which can be drawn from these experiments, I need say very few words. I can now have no doubt of the fact that "bacteriums" can be produced in hermetically-sealed vessels containing an infusion of organic matter, whether animal or vegetable, though supplied only with air passed through a red-hot tube with all necessary precautions for ensuring the thorough heating of every portion of it, and though the infusion itself be thoroughly boiled. But how far this fact affects the question of what is called "spontaneous generation" is quite another matter.

It seems clear that either (1) the germs of bacterium are capable of resisting the boiling temperature in a fluid, or (2) they are spontaneously generated, or (3) they are not "organisms" at all. I was myself somewhat inclined to the latter belief concerning them at one time; but some researches on which I am now engaged have gone far to convince me that they are really minute vegetable forms.

The choice therefore seems to remain between the other two conclusions. Upon these I will not venture a positive opinion, but remark only, that if it be true that "germs" can resist the boiling temperature in fluid, then both parties in the controversy are working upon a false principle, and neither M. Pouchet nor M. Pasteur is likely at present to solve the question of spontaneous generation. In truth, if M. Pasteur's facts are incorrect, the whole question is relegated to the domain of what the French Academy Commission calls "pure discussion;" and the one point which I claim to have established by their researches is precisely that M. Pasteur's facts are inexact—not because his experiments were not most admirably performed, but simply because the magnifying power of his microscope was insufficient for the work to which he applied it. I desire to append two remarks to this paper. The first is, that the common *à priori* objection, which M. Pasteur so well expressed in his memoir, to heterogeny in all forms, viz. that it is a doctrine which has been gradually driven from all the higher forms of life

in exact proportion as our observation of them has become more exact, until at last it has been compelled to take refuge in those lowest forms which we are almost or altogether unable to observe, is really of little or no force. Its cogency depends on analogy, and the analogy has no existence. It is quite equally to be expected *à priori* that if any forms of life are generated spontaneously, they will be the very lowest and simplest forms, and since these happen to be also the most minute, the objection loses its whole force. And it is also a thing to be expected that we should find only the lowest forms, the earliest, *i. e.* in the scale of existence, produced under the disadvantageous circumstances in which they must be placed in such experiments as those above detailed.

The other remark is this, that, so far as my present researches have led me, I cannot but look upon improvement in the construction of microscopes, and increase of their power, as the only way in which our means of investigation of such questions as the production of Bacterium is likely to be largely increased. The $\frac{1}{50}$ object-glass recently constructed by Messrs. Powell and Lealand, of which a notice has appeared in the Proceedings of the Royal Society, has already shown something like an appearance of structure in these minute objects, and leaves, I think, no doubt about their organic character.

II. "On the Magnetic Character of the Iron-built Armour-plated Battery 'Pervenetz' of the Imperial Russian Navy." By Capt. J. Belavenetz, R.I.N., Superintendent of the Compass Observatory at St. Petersburg. Communicated by Archibald Smith, M.A., Corresponding Member of the Scientific Committee of the Imperial Russian Navy. Received March 23, 1865.

1. The 'Pervenetz' is an iron-built armour-plated ship of war, constructed for the Russian Government by the Thames Iron and Ship-Building Company at their works at Blackwall.

2. The following are her dimensions:—

Length....................	220 feet
Breadth	53 feet
Depth of hold	26·6 feet
Builder's measurement	2393 tons
Horse-power	300

3. The upper and main decks were plated with sheet iron.

4. The greater part of the side plating was fixed in England and while the observations recorded in the paper were made, but no part of the end plating was fixed, the plates being taken on board and carried as cargo.

The direction of her head in building was S. 22° 17′ W. magnetic.

5. The author was commissioned by the Russian Government to superintend the compass equipment and compass correction of the ship. He

arrived in England a few days before the launch, and immediately undertook the observation of the deviation and the horizontal and vertical force at various selected positions in the ship. These were the following, viz. five positions on the upper deck, about 4 ft. 6 in. above the deck, distinguished as *b, c, d, e, f,* going from stern to bow; *b* abaft the mizen-mast, *c* immediately before the mizen-mast, *d* before the main-mast, *e* about the middle of the length, and *f* before the fore-mast: three on the main deck, *g, k, l*; *g* halfway between the mizen and mainmasts, *k* below *d*, and *l* below *f*: two in the hold, near the centre of the ship, *m* and *n*: *b* being near the place where the neutral plane or rather the surface separating the part of the ship which displayed north magnetism from that which displayed south magnetism intersected the deck, was selected as a place for the standard compass, and a place was prepared for it by making there a false hatchway or compass platform, by replacing an iron beam by a wooden beam, and substituting wood planks for iron plates at that part of the deck.

6. From the observations, using the notation of the 'Admiralty Manual' (which has been translated into Russian by the author for the use of the Imperial Navy), by estimating the value of $\mathfrak{D}$ and λ, and making use of the formulæ of the Manual, 2nd edit., p. 110,

$$\mathfrak{B}=\frac{H'}{\lambda H}\cos\zeta'-(1+\mathfrak{D})\cos\zeta,$$

$$\mathfrak{C}=-\frac{H'}{\lambda H}\sin\zeta'+(1-\mathfrak{D})\sin\zeta,$$

the author obtained from the observations made the following results:—

	Place of Compass.	Deviation.	Hor. force, $\frac{H'}{H}$.	Vert. force, $\frac{Z'}{Z}$.	Assumed.		Computed.	
					$\mathfrak{D}$.	λ.	$\mathfrak{B}$.	$\mathfrak{C}$.
Shore.	*a*		1	1				
Upper deck.	*b*	3° 55′ W.	·286	·700	·100	·800	·700	–·170
	c	1° 36′ W.	·247	·906	·150	·800	·782	–·190
	d	9° 20′ W.	·191	1·168	·150	·820	·855	–·200
	e	30° 6′ W.	·139	1·332	·150	·820	·955	–·200
	f	28° 58′ W.	·169	1·971	·150	·820	·930	–·158
Main deck.	*g*	16° 27′ E.	·130	·134	·186	·790	·932	–·293
	k	14° 41′ E.	·230	·156	·186	·790	·807	–·270
	l	11° 11′ E.	·058	·154	·186	·790	1·025	–·286
Hold.	*m*	15° 11′ E.	·138	·074	·200	·700	·910	–·270
	n	17° 41′ E.	·216	·059	·200	·700	·802	–·281

7. These show—

(1) The small deviations on the stocks of the compasses in every position, except near the north end of the ship, where, probably from the armour-plating not being fixed, irregular deviations were to be expected.

(2) The great diminution of horizontal force, especially near the south end.

(3) The very great increase of vertical force near the south end on the upper deck, and the great diminution of the vertical force in descending. From these we infer that there would be a very large heeling error to windward at *f*, and a large heeling error to leeward on the main deck.

(4) The very large amount of semicircular deviation, both as regards $\mathfrak{B}$ and $\mathfrak{C}$, the last having a large negative value in consequence of the port-side of the vessel having been to the south in building.

8. The best place for the compass is point *b*, near which the standard compass was afterwards fixed.

9. We also see that at the point *l* the deviations exceed 180°. It is to be observed, however, that from the armour-plating not being fixed, we must not accept this as an instance of what the deviation would be in a completed vessel.

10. The 'Pervenetz' was launched on the 21st of May, and on the same day moved to the Victoria Docks. On the way and in the dock the deviations were observed at the point *b*, and the following values of $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$ were obtained:—

$$\mathfrak{B}=+\cdot 639, \qquad \mathfrak{C}=-\cdot 136. \qquad \mathfrak{D}=\cdot 100.$$

11. On the 22nd of May she was moved in the dock, and the following observations were made at the point *b*:—

Ship's head by Compass.	Deviation.	Hor. force, $\frac{H'}{H}$.
S. 46° E.	40° 30′ E.	·606
N. 27° W.	18° 00′ W.	1·419

from which the following values of the coefficients are derived:—

$$\mathfrak{B}=+\cdot 736, \qquad \mathfrak{C}=-\cdot 122, \qquad \mathfrak{D}=\cdot 126, \qquad \lambda=\cdot 806.$$

12. Observations at the other positions showed that the assumed values of λ and $\mathfrak{D}$ did not differ much from the truth.

13. The vessel lay in the Victoria Docks till the 27th of July, with her head very nearly N. 55° E. At the end of that time the coefficients were—

July 27, 1863.	Deviation. Head N. 55° E.	$\frac{H'}{H}$.	$\frac{Z'}{Z}$.	$\mathfrak{D}$.	λ.	$\mathfrak{B}$.	$\mathfrak{C}$.
b	18° 50′ E.	·879	1·039	·083	·850	+·210	+·140
c	26° 50′	1·089	·994	·150	·800	+·541	+·060
d	21° 10′	·890	1·219	·150	·820	+·241	+·098
e	35° 10′	1·117	1·061	·150	·820	+·618	+·258
f	26° 20′	·960	1·494	·150	·820	+·400	+·114
g	30° 00′	·904	·045	·186	·790	+·357	+·190
k	26° 30′	·973	·134	·186	·790	+·400	+·087
l	33° 47′	·913	−·012	·186	·790	+·391	+·260

14. Comparing these values with those in Table I., we see the great changes which the magnetism of the ship had undergone in the Victoria Docks.

15. The ship having her head to the east instead of to the west, all the $\mathfrak{C}$'s had changed from $-$ to $+$, and having had her head to north instead of south, all the $\mathfrak{B}$'s had diminished.

16. From the same cause we see that Z at the stern increases, at the bow diminishes; at *d* there is an increase of Z, owing to the machinery under that part of the deck.

17. At one point in the main deck Z is negative, showing that the upward vertical force of the ship was greater than the vertical force of the earth. The great apparent change in H arises from the part of the ship which was south in building, and which consequently attracted the north end of the ship, having been directed to the north, and increasing instead of diminishing the directive force.

18. The force of the ship was greatest when the ship was first placed with her head in the new position, and greatly diminished afterwards, as will be seen from the following Table.

Value of $\frac{H'}{H}$, ship's head at N. 55° E.

Compass.	*b.*	*c.*	*d.*	*f.*	*g.*	*l.*
1863.						
June 2 ..	1·296	1·382	....	....	....	....
„ 15 ..	1·079	1·139	....	1·316	....	1·301
„ 29 ..	1·016	1·075	1·114	....	....	1·187
July 13 ..	1·016	1·051	1·040	....	·952	1·040
„ 27 ..	·879	1·089	·890	·960	·904	·913

19. On leaving the Thames for the Baltic the 'Pervenetz' had five compasses:—

1. The Standard, in the position *b*, but 7 feet 6 in. above the upper deck.
2. The Bridge Compass.

3 & 4. Two Binnacle Compasses.

5. The Main-deck Compass.

20. The following Table of the Coefficients is derived from observations made in the Thames on the 3rd of August.

Compass.	A. $\mathfrak{A}$.	B. $\mathfrak{B}$.	C. $\mathfrak{C}$.	D. $\mathfrak{D}$.	E. $\mathfrak{E}$.	λ.	μ.	Part of D from fore-and-aft induction.	Part of D from transverse induction.	Heeling error.
Standard	−19′ −·005	+16° 59′ +·305	+2° 29′ +·040	+5° +·087	−1′ ·000	·818	1·000	−3° 52′	+8° 52′	+ 48′
Bridge	−33′ −·009	+24° 18′ +·424	−1° 14′ −·020	+2° 52′ +·050	−32′ −·009	·841	1·186	−4 2	+6 54	+1 6
Starboard-steering	+8′ +·002	+31° 16′ +·549	+5° 14′ +·083	+5° 17′ +·094	+51′ +·014	·792	1·017	−4 51	+10 18	− 15
Port-steering ..	−18′ −·005	+31° 45′ +·558	−1° 32′ −·026	+5° 50′ +·101	−51′ −·014	·780	1·057	−5 14	+11 4	− 10
Main Deck	−1° 40′ −·209	+29° 4′ +·504	+7° 19′ +·119	3° 40′ ·063	−53′ −·016	·752	·038	−7 44	+11 21	−2 15

21. The small amount of the $\mathfrak{D}$, compared to that in the iron-built armour-plated ships of the Royal Navy, is remarkable. It is no doubt

to be attributed partly to the want of the transverse armour-plating at the extremities, and to the comparatively small number of bulkheads giving a smaller $-e$ than in the iron-plated ships of the Royal Navy, and partly to the armour-plating of the sides being continued on each side of the compass giving a large $-a$, and in this respect resembling the effect of the armour-plating in the Royal Oak class of ships in the Royal Navy.

22. The large amount of the heeling error in the Main-deck Compass and its direction is remarkable.

23. The 'Pervenetz' sailed for the Baltic on the 8th of August, 1863.

24. The only change in her magnetism on the voyage was an increase in the $+\mathfrak{C}$, which was no doubt owing to the starboard side being south.

25. The principal practical conclusions to be derived from the observations in the 'Pervenetz' seem to be,—

(1) That iron ships should be built head south.

(2) That in whatever direction an iron armour-plated ship is built, she ought to be placed in the opposite direction while plating, so as to reduce the semicircular deviation as much as possible. This results also from the observations made in the ships of the Royal Navy; but the plan of plating a ship in the opposite direction to that of building was first practised intentionally, and with the design of reducing the semicircular deviation, in the 'Pervenetz,' and, as will have been seen, with complete success.

(3) That great and rapid changes take place in the semicircular deviation some time after launching.

(4) The great amount of information both as to the semicircular deviation and the heeling error, which can be obtained by appropriate observations made while the vessel is building.

(5) The importance for this and other purposes, of reducing and recording the deviations of all iron ships, so as to obtain the values of the coefficients, and particularly λ and $\mathfrak{D}$, and to be able to estimate them in any new ship of the same class.

(6) The great importance of preparing a proper place for the reception of the Standard Compass in the construction of an iron ship.

III. "Notes of Researches on the Acids of the Lactic Series.—No. V. Action of Zinc upon a mixture of Ethyl Oxalate and Amyl Iodide." By Edward Frankland, F.R.S., and B. F. Duppa, Esq. Received March 30.

When a mixture of equivalent proportions of ethyl oxalate and amyl iodide is digested with granulated zinc at 70° C., the zinc is gradually dissolved, while much hydride of amyl and amylene are given off. The mixture finally assumes a viscous or semisolid condition, and, when treated with water, produces a further quantity of hydride of amyl, which distils

off at a gentle heat. On the subsequent application of a higher temperature, water, accompanied by amylic alcohol, amyl iodide, and an ethereal liquid, distils over, the three latter forming a mixture, the separation of which into its component parts presents rather formidable difficulties. After drying with chloride of calcium, this oily mixture begins to boil at about 132° C.; the product first passing over consists principally of amylic alcohol, mixed with amyl iodide. Afterwards the thermometer rapidly rises to 200° C., between which temperature and 205° C. a considerable section of the remaining liquid, which we will call A, passes over. There then occurs a further rapid rise of temperature until the thermometer remains stationary between 222° and 226°. The section collected between these points we will call B. Finally, the temperature rises to 260°–264°, between which points the remaining liquid (C) passes over. By repeated fractional distillation, the larger portion of the section A was obtained at the nearly fixed boiling-point of 203° C. This liquid was submitted to analysis, and yielded numbers closely corresponding with the formula

$$C_9\,H_{18}\,O_3.$$

Interpreted by further results detailed below, this formula resolves itself into

$$C_2\left\{\begin{array}{l} C_5\,H_{11} \\ \;H \\ \,O\,H \\ \hline \;O \\ \,OC_2\,H_5. \end{array}\right.$$

The ethereal body with the lowest boiling-point produced in this reaction is therefore *ethylic amylohydroxalate,* or oxalic ether in which one atom of oxygen is replaced by one atom of amyl and one of hydrogen. This body also stands in very close relation to lactic ether; for if the atom of methyl in lactic ether be replaced by amyl, ethylic amylohydroxalate is produced.

$$\begin{array}{cc} C_2\left\{\begin{array}{l} CH_3 \\ H \\ OH \\ \hline O \\ OC_2\,H_5 \end{array}\right. & \qquad C_2\left\{\begin{array}{l} C_5\,H_{11} \\ H \\ OH \\ \hline O \\ OC_2\,H_5. \end{array}\right. \\ \text{Lactic ether.} & \qquad \text{Ethylic amylohydroxalate.} \end{array}$$

The two stages in the production of ethylic amylohydroxalate are explained by the following equations:—

$$C_2\left\{\begin{array}{l} O \\ O\,C_2\,H_5 \\ \hline O \\ O\,C_2\,H_5 \end{array}\right. + Zn_5'' + 4\left.\begin{array}{c} C_5H_{11} \\ I \end{array}\right\} = C_2\left\{\begin{array}{l} C_5\,H_{11} \\ Zn\,C_5\,H_{11} \\ O\,Zn\,C_5\,H_{11} \\ \hline O \\ O\,C_2\,H_5 \end{array}\right. + Zn\left\{\begin{array}{l} C_5\,H_{11} \\ OC_2H_5 \end{array}\right. + 2\,Zn''\,I_2$$

$$C_2\left\{\begin{array}{l} C_5\,H_{11} \\ Zn\,C_5\,H_{11} \\ O\,Zn\,C_5\,H_{11} \\ \hline O \\ O\,C_2\,H_5 \end{array}\right. + 3H_2O = \underset{\text{Ethylic amylo-hydroxalate.}}{C_2\left\{\begin{array}{l} C_5\,H_{11} \\ H \\ O\,H \\ \hline O \\ O\,C_2\,H_5 \end{array}\right.} + \underset{\text{Zinc hydrate.}}{2\,Zn\left\{\begin{array}{l} O\,H \\ O\,H \end{array}\right.} + \underset{\text{Amyl hydride.}}{\left.\begin{array}{c} C_5\,H_{11} \\ H \end{array}\right\}} + \underset{\text{Amylic alcohol.}}{\left.\begin{array}{c} C_5\,H_{11} \\ H \end{array}\right\}O.}$$

We have not attempted to give a name to the body from which ethylic amylohydroxalate is directly produced by the action of water, as shown in the last of the foregoing equations. The resources of chemical nomenclature, already too severely taxed, would scarcely be able to elaborate a rational name for this body, which consists of oxalic ether wherein an atom of oxygen is replaced, half by amyl and half by zincmonamyl, whilst a second atom of zincmonamyl is substituted for one of ethyl.

Ethylic amylohydroxalate is a somewhat oily, transparent, and slightly straw-coloured liquid of specific gravity ·9449 at 13° C., possessing a pleasant aromatic odour and a burning taste. It boils at 203° C., and has a vapour-density of 5·47. The above formula requires 6·0, which is removed to an unusual extent from the experimental number. To this discrepancy we shall again refer presently.

Section B of the oily liquid, after careful rectification, gave a product boiling at 224–225°, and yielded on analysis results agreeing with the formula

$$C_{11}\,H_{22}\,O_3.$$

This formula might be interpreted as that of *ethylic amyloethoxalate*, the rational formula of which would be

$$C_2\left\{\begin{array}{l} C_5\,H_{11} \\ C_2\,H_5 \\ \underline{O\,H} \\ O \\ OC_2\,H_5. \end{array}\right.$$

We were at first inclined to regard this as the actual constitution of the new ether, believing it to be possible that ethylic oxalate and amylic iodide mutally decomposed each other, producing a mixture of amylic and ethylic oxalates with the iodides of amyl and ethyl. An analogous decomposition of mixed ethereal salts of oxygen acids has been recently noticed, but the test of experiment obliged us to abandon this view of the reaction. We found, it is true, a remarkable depression of temperature, amounting to 9·3° C., on mixing one atom of ethyl oxalate with one of amylic iodide, but on submitting the mixture to distillation, the thermometer rose to the boiling-point of amylic iodide (147°) before ebullition commenced; thus showing that none of the much more volatile ethylic iodide had been formed. No transfer of radicals therefore takes place when ethylic oxalate is heated with amylic iodide, and consequently no zincethyl can be formed when this mixture is acted upon by zinc. We therefore prefer to view the ether now under consideration as *ethylic ethyl-amylhydroxalate*, analogous in constitution to Wurtz's ethylic ethyl lactate.

$$C_2\left\{\begin{array}{l} C\,H_3 \\ H \\ \underline{O\,C_2\,H_5} \\ O \\ O\,C_2\,H_5. \end{array}\right. \qquad\qquad C_2\left\{\begin{array}{l} C_5\,H_{11} \\ H \\ \underline{O\,C_2\,H_5} \\ O \\ O\,C_2\,H_5. \end{array}\right.$$

Ethylic ethyl lactate. Ethylic ethyl-amylhydroxalate.

On this view, the following equations represent the formation of this ether.

$$C_2\left\{\begin{array}{l}O\\ OC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right. + Zn''_4 + 4\,\begin{array}{c}C_5H_{11}\\ I\end{array}\Big\{ = C_2\left\{\begin{array}{l}C_5H_{11}\\ Zn\,C_5H_{11}\\ OC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right. + Zn''\left\{\begin{array}{l}C_5H_{11}\\ OC_5H_{11}\end{array}\right. + 2Zn\,I_2.$$

Ethyl oxalate. Iodide of amyl. Zinc amylo-amylate.

$$C_2\left\{\begin{array}{l}C_5H_{11}\\ Zn\,C_5H_{11}\\ OC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right. + 2H_2O = C_2\left\{\begin{array}{l}C_5H_{11}\\ H\\ OC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right. + \left.\begin{array}{c}C_5H_{11}\\ H\end{array}\right\} + Zn''\left\{\begin{array}{l}OH\\ OH.\end{array}\right.$$

Ethylic ethyl-amylhydroxalate. Hydride of amyl.

Ethylic ethyl-amylhydroxalate is a straw-coloured oily liquid, possessing an aromatic but somewhat amylic odour and a burning taste. Its specific gravity was found to be ·9399 at 13° C. It boils between 224° and 225° C. A determination of the sp. gr. of its vapour by Gay-Lussac's method gave the number 6·29, the above formula requiring 6·92.

Section C of the oily product, boiling about 262° C., was next submitted to investigation. It gave results on analysis agreeing well with the formula

$$C_{14}H_{28}O_3.$$

The body is therefore ethylic diamyloxalate, the normal homologue of leucic ether, as seen from the following comparison:—

$$C_2\left\{\begin{array}{l}(C_2H_5)_2\\ OH\\ \hline O\\ OC_2H_5\end{array}\right. \qquad\qquad C_2\left\{\begin{array}{l}(C_5H_{11})_2\\ OH\\ \hline O\\ OC_2H_5\end{array}\right.$$

Leucic ether. Ethylic diamyloxalate.

The production of ethylic diamyloxalate is explained by the following equations:—

$$C_2\left\{\begin{array}{l}O\\ OC_2H_5\\ \hline O\\ OC_2H_5\end{array}\right. + Zn_4'' + 4\left.\begin{array}{c}C_5H_{11}\\ I\end{array}\right\} = C_2\left\{\begin{array}{l}(C_5H_{11})_2\\ O\,Zn\,C_5H_{11}\\ \hline O\\ OC_2H_5\end{array}\right. + Zn''\left\{\begin{array}{l}C_5H_{11}\\ OC_2H_5\end{array}\right. + 2Zn''I_2.$$

Ethylic oxalate. Amyl iodide. Ethylic zinc-monamyl-diamyloxalate. Zinc amylo-ethylate.

$$C_2\left\{\begin{array}{l}(C_5H_{11})_2\\ O\,Zn\,C_5H_{11}\\ \hline O\\ OC_2H_5\end{array}\right. + 2H_2O = C_2\left\{\begin{array}{l}(C_5H_{11})_2\\ OH\\ \hline O\\ OC_2H_5\end{array}\right. + \left.\begin{array}{c}C_5H_{11}\\ H\end{array}\right\} + Zn''\left\{\begin{array}{l}OH\\ OH.\end{array}\right.$$

Ethylic zincmonamyl-diamyloxalate. Ethylic diamyl-oxalate.

Ethylic diamyloxalate closely resembles the two foregoing ethers in its appearance and properties. It is, however, a thicker oil, and flows less readily, and it has the lowest specific gravity of any ether belonging to this series, its density at 13° C. being only ·9137. The following comparison of the specific gravities of all the ethers of this series shows that they generally increase inversely as their atomic weights.

Name.	Formula.	Sp. gr.	Temp.
Ethylic lactate	$C_5\ H_{10}\ O_3$	1·042	13°
Ethylic dimethoxalate	$C_6\ H_{12}\ O_3$	0·9931	13
Ethylic ethyl lactate	$C_7\ H_{14}\ O_3$	0·9203	0
Ethylic ethomethoxalate	$C_7\ H_{14}\ O_3$	0·9768	13
Methylic diethoxalate	$C_7\ H_{14}\ O_3$	0·9896	16·5
Ethylic diethoxalate	$C_8\ H_{16}\ O_3$	0·9613	18·7
Ethylic amylhydroxalate	$C_9\ H_{18}\ O_3$	0·9449	13
Ethylic ethyl-amylhydroxalate	$C_{11}\ H_{22}\ O_3$	0·9399	13
Amylic diethoxalate	$C_{11}\ H_{22}\ O_3$	0·9322	13
Ethylic diamyloxalate	$C_{14}\ H_{28}\ O_3$	0·9137	13

Ethylic diamyloxalate boils at about 262°, and distils with little or no change. The specific gravity of its vapour was found to be only 5·9 instead of 8·4. The investigation of these ethers has revealed a tendency to dissociation increasing with the weight of the atoms replacing the atom of oxygen in oxalic ether. Thus, beginning with lactic ether, which has the normal vapour-density, we find a gradual divergence culminating in ethylic diamyloxalate, as seen from the following series of numbers:—

Name.	Formula.	Vapour-densities.	
		Calculated.	Found.
Ethylic lactate	$C_5\ H_{10}\ O_3$	4·07	4·14
Ethylic dimethoxalate	$C_6\ H_{12}\ O_3$	4·56	4·67
Ethylic ethyl lactate	$C_7\ H_{14}\ O_3$	5·03	5·052
Ethylic ethomethoxalate	$C_7\ H_{14}\ O_3$	5·03	4·98
Methylic diethoxalate	$C_7\ H_{14}\ O_3$	5·03	4·84
Ethylic diethoxalate	$C_8\ H_{16}\ O_3$	5·528	5·24
Ethylic amylhydroxalate	$C_9\ H_{18}\ O_3$	6·01	5·47
Ethylic ethyl-amylhydroxalate	$C_{11}\ H_{22}\ O_3$	6·92	6·29
Amylic diethoxalate	$C_{11}\ H_{22}\ O_3$	6·92	6·74
Ethylic diamyloxalate	$C_{14}\ H_{28}\ O_3$	8·4	5·9

We have likewise prepared the acids corresponding to the three ethers above mentioned. The first is obtained by decomposing ethylic amylhydroxalate with baryta, treating the solution of the barium-salt thus obtained with excess of sulphuric acid, and then dissolving out the organic acid with ether. On evaporating the ethereal solution, the acid remains as a thick oil which does not crystallize after several days' exposure over sulphuric acid *in vacuo*. The calcium- and barium-salts form white crystalline masses, which exhibit respectively the composition

$$C_4\left\{\begin{array}{l}(C_5\,H_{11})_2\\ H_2\\ \underline{(O\,H)_2}\\ O_2\\ \left.\begin{array}{l}O\\ O\end{array}\right\}Ca''\end{array}\right. \quad \text{and} \quad C_4\left\{\begin{array}{l}(C_5\,H_{11})_2\\ H_2\\ \underline{(O\,H)_2}\\ O_2\\ \left.\begin{array}{l}O\\ O.\end{array}\right\}Ba''\end{array}\right.$$

We have also obtained a beautifully crystalline acid of the same composition, and perfectly pure, from its zinc-salt, which is contained in the residue remaining after the distillation of the three ethers above described. *Amylhydroxalic acid* prepared from this zinc-salt is but sparingly soluble in water, from which, however, it crystallizes in magnificent nacreous scales that fuse at 60°·5 C., but afterwards remain liquid for some time even at ordinary temperatures; they are very unctuous to the touch, and readily soluble in alcohol and ether. Several analyses gave results closely corresponding with the formula

$$C_2\left\{\begin{array}{l} C_5\,H_{11}\\ H\\ \underline{O\,H}\\ O\\ O\,H.\end{array}\right.$$

The barium-salt of this acid crystallizes in large and beautiful nacreous scales like paraffin, tolerably soluble in water, and exhibiting the composition

$$C_4\left\{\begin{array}{l} (C_5\,H_{11})_2\\ H_2\\ \underline{(O\,H)_2}\\ O_2\\ O\\ O.\end{array}\right.\!\!Ba''$$

We have also prepared a copper salt which is deposited from its aqueous solution in the form of minute light-blue scales, very sparingly soluble in water. The numbers obtained by the analysis of this salt agree with the formula

$$C_4\left\{\begin{array}{l} (C_5\,H_{11})_2\\ H_2\\ \underline{(O\,H)_2}\\ O_2\\ O\\ O.\end{array}\right.\!\!Cu''$$

By the decomposition of ethylic ethyl-amylhydroxalate with alcoholic potash, subsequent addition of sulphuric acid in excess, and treatment with ether, the corresponding acid was obtained as a thick oil, gradually solidifying to a crystalline mass, which, however, did not appear to be in a fit state for the determination of its fusing-point. We have examined the barium- and silver-salts of this acid, which have respectively the following composition:—

$$C_4\left\{\begin{array}{l} (C_5\,H_{11})_2\\ H_2\\ \underline{(O\,C_2\,H_5)_2}\\ O_2\\ O\\ O\end{array}\right.\!\!Ba'' \quad \text{and} \quad C_2\left\{\begin{array}{l} C_5\,H_{11}\\ H\\ \underline{OC_2\,H_5}\\ O\\ O\,Ag\end{array}\right.$$

Barium ethyl-amylhydroxalate. Silver ethyl-amylhydroxalate.

Ethylic diamyloxalate is readily decomposed by baryta-water. After removing the excess of baryta in the usual manner, barium diamyloxalate

crystallizes on evaporation in minute elastic needles, which, when dry, have the appearance of wool. It is moderately soluble in hot water, but sparingly so in cold. This salt gave numbers on analysis closely corresponding with the formula

$$C_4\left\{\begin{array}{l}(C_5H_{11})_4\\(OH)_2\\\hline O_2\\\left.\begin{array}{l}O\\O.\end{array}\right\}Ba''\end{array}\right.$$

If barium diamyloxalate be dissolved in hot dilute alcohol, and excess of sulphuric acid be added, the liquid after filtration contains diamyloxalic acid in solution. On heating upon a water-bath, the alcohol gradually evaporates, and diamyloxalic acid crystallizes in the hot solution as a beautiful network of brilliant silky fibres, which, after being well washed in cold water and dried at 100°, yielded on analysis numbers closely corresponding with the formula

$$C_2\left\{\begin{array}{l}(C_5H_{11})_2\\OH\\\hline O\\OH.\end{array}\right.$$

Diamyloxalic acid presents the appearance of colourless satiny fibres, which are insoluble in water, but soluble in alcohol and ether. This acid is remarkable for its high melting-point, 122° C., in which respect it surpasses any of the acids of this series. Its melting-point is very sharply defined, and it solidifies immediately on a very slight reduction of temperature. Heated more strongly, it sublimes and condenses on a cold surface in white crystalline flakes like snow.

No. VI. *Action of Zinc upon Amylic Oxalate and Ethylic Iodide.*

Equivalent proportions of amylic oxalate and ethylic iodide were digested at 50° to 60° C., with excess of granulated zinc, for several days. The reaction proceeded with extreme sluggishness, and was not completed before the expiration of a week. Being then mixed with water and submitted to distillation, an oily liquid passed over, which, on rectification, was ultimately resolved into amylic alcohol and an ethereal liquid, which analysis proved to be *amylic leucate.*

The two consecutive reactions by which this body is produced are expressed in the following equations:—

$$\underset{\text{Amylic oxalate.}}{C_2\left\{\begin{array}{l}O\\OC_5H_{11}\\\hline O\\OC_5H_{11}\end{array}\right.}+Zn_4''+4\left.\begin{array}{c}C_2H_5\\I\end{array}\right\}=\underset{\text{Amylic zincmonethyl leucate.}}{C_2\left\{\begin{array}{l}(C_2H_5)_2\\OZnC_2H_5\\\hline O\\OC_5H_{11}\end{array}\right.}+\underset{\text{Zinc ethylo-amylate.}}{Zn\left\{\begin{array}{l}C_2H_5\\OC_5H_{11}\end{array}\right.}+2ZnI_2.$$

$$\underset{\text{Amylic zincmonethyl leucate.}}{C_2\left\{\begin{array}{l}(C_2H_5)_2\\OZnC_2H_5\\\hline O\\OC_5H_{11}\end{array}\right.}+2H_2O=\underset{\text{Amylic leucate.}}{C_2\left\{\begin{array}{l}(C_2H_5)_2\\OH\\\hline O\\OC_5H_{11}\end{array}\right.}+\left.\begin{array}{c}C_2H_5\\H\end{array}\right\}+Zn\left\{\begin{array}{l}OH\\OH.\end{array}\right.$$

Amylic leucate is a colourless, transparent, and slightly oily liquid, possessing a fragrant odour of a somewhat amylic character. It is insoluble in water, but miscible in all proportions with alcohol and ether. Its specific gravity is ·93227 at 13° C. It boils constantly at 225° C., and its vapour has a density of 6·74 (theoretical 6·97).

The boiling-point and specific gravity, in the liquid form, of amylic leucate and its isomer, ethylic amylethoxalate, are almost absolutely identical. Leucate of amyl is readily decomposed by either aqueous or alcoholic solutions of the alkalies, or by baryta-water, yielding amylic alcohol and a leucate of the base.

No. VII. *Action of Zinc upon a mixture of Amyl Oxalate and Amyl Iodide.*

When equivalent proportions of the amyl iodide and amyl oxalate are heated gently in contact with zinc, a brisk reaction soon sets in. After evolving much hydride of amyl and amylene, the whole solidifies to a gum-like mass, which, on distillation with water, yields an oily liquid resembling that obtained when ethyl oxalate is employed. We have every reason to believe that the same series of ethers as those described in note No. V. are here produced, with the difference that they are amylic, instead of ethylic ethers. This difference of base, however, renders it impossible successfully to separate these ethers from each other, their boiling-points being so high as to determine decomposition when their distillation is attempted. We might, it is true, have decomposed the mixed ethers with solution of baryta, and thus have obtained the mixed acids, but the task of disentangling the latter appeared also so hopeless, that we have not attempted it.

IV. "Notes of Synthetical Researches on Ethers.—No. I. Synthesis of Butyric and Caproic Ethers from Acetic Ether." By Edward Frankland, F.R.S., and B. F. Duppa, Esq. Received April 5, 1865.

For some time past we have been engaged in the study of the consecutive action of sodium and the iodides of methyl and ethyl upon acetic ether. When iodide of methyl is used, the chief products of the reaction are two ethereal bodies possessing respectively formulæ, which we will provisionally write as follows:—

$$\left.\begin{matrix} C\,O'' \\ C_4\,H_7 \\ C_2\,H_5 \end{matrix}\right\} O_2 \quad \text{and} \quad \left.\begin{matrix} C\,O'' \\ C_5\,H_9 \\ C_2\,H_5 \end{matrix}\right\} O_2.$$

These bodies are decomposed, even in the cold, by baryta-water, yielding barium carbonate, alcohol, and two new ethereal liquids having formulæ which, without expressing any opinion as to their nature or constitution, may be thus written:—

$$\left.\begin{matrix} C_4 H_7 \\ H \end{matrix}\right\} O \quad \text{and} \quad \left.\begin{matrix} C_5 H_9 \\ H \end{matrix}\right\} O.$$

We have also obtained corresponding results by the employment of iodide of ethyl in place of iodide of methyl, and are now occupied in the preparation of a paper containing the details of this investigation, which we hope very soon to have the honour of laying before the Royal Society*. In the meantime, however, some of our results are so remarkable that we hasten to communicate them at once in this preliminary note.

It has been proved by Kolbe and Frankland, nearly twenty years ago, that methyl is a constituent of acetic acid†, and in the year 1857 these chemists were the first to propose the derivation of this and a large number of other organic compounds from the carbonic acid or tetratomic carbon type‡. According to this view, which is now gradually receiving the assent of chemists, the rational formula of acetic ether is

$$C\left\{\begin{matrix} C H_3 \\ O \\ O C_2 H_5, \end{matrix}\right.$$

or, with the formula of the contained atom of methyl fully developed,

$$C\left\{\begin{matrix} C\left\{\begin{matrix} H \\ H \\ H \end{matrix}\right. \\ O \\ O C_2 H_5. \end{matrix}\right.$$

Thus the radical methyl, in acetic ether, contains three single atoms of hydrogen, combined with a tetratomic atom of carbon. If one of these atoms of hydrogen be replaced by methyl, an ether, having the composition of propionic ether, will obviously be produced:

$$C\left\{\begin{matrix} C\left\{\begin{matrix} C H_3 \\ H \\ H \end{matrix}\right. \\ O \\ O C_2 H_5. \end{matrix}\right.$$

If a second atom of hydrogen be replaced by another atom of methyl, butyric ether or its isomer will, in like manner, be formed:

$$C\left\{\begin{matrix} C\left\{\begin{matrix} C H_3 \\ C H_3 \\ H \end{matrix}\right. \\ O \\ O C_2 H_2. \end{matrix}\right.$$

* Whilst engaged in these experiments we became aware, through the 'Jahresbericht der Chemie,' that the reaction had already been studied by Geuther, who, however, owing to his having conducted the process in a somewhat different manner, obtained only two of the compounds above mentioned, viz. the body $C_7 H_{12} O_3$ by the action of sodium and iodide of methyl upon acetic ether, and the compound $C_8 H_{14} O_3$ in the corresponding reaction with iodide of ethyl.

† Memoirs and Proceedings of the Chem. Soc. vol. iii. p. 386; and Ann. der Ch. und Pharm. Bd. lxv. S. 288, und Bd. lxix. S. 258.

‡ Ann. der Ch. und Pharm. Bd. ci. S. 260.

An ether, of the same composition as the last, will also obviously result, if, instead of replacing two atoms of hydrogen by two of methyl, one of those atoms be substituted by one of ethyl,

$$C\left\{\begin{array}{l} C\left\{\begin{array}{r} C_2H_5\\ H\\ H\end{array}\right.\\ O\\ O\,C_2H_5.\end{array}\right.$$

Again, if two atoms of hydrogen in the methyl of acetic ether be replaced by two of ethyl, caproic ether should result:

$$C\left\{\begin{array}{l} C\left\{\begin{array}{r} C_2H_5\\ C_2H_5\\ H\end{array}\right.\\ O\\ O\,C_2H_5.\end{array}\right.$$

And, finally, if all three atoms of hydrogen be replaced by amyl, there must be produced the ether of an acid possessing the atomic weight of margaric acid:

$$C\left\{\begin{array}{l} C\left\{\begin{array}{r} C_5H_{11}\\ C_5H_{11}\\ C_5H_{11}\end{array}\right.\\ O\\ O\,C_2H_5.\end{array}\right.$$

It is unnecessary to follow theoretically these reactions further; but it is obvious, from what has been already advanced, that, by a proper selection of the three radicals put into the place of the methylic hydrogen, any ether, from the margaric downwards, can be produced at will, by a process analogous to that which we have experimentally demonstrated in the lactic series.

The present note describes the method by which we have already realized several of these substitutions.

Synthesis of Butyric Ether.

When sodium is gently heated with acetic ether, it gradually dissolves with evolution of hydrogen, and on cooling, the liquid solidifies to a crystalline mass, which becomes hot when mixed with iodide of ethyl, abundance of iodide of sodium being formed: nevertheless it is advisable to complete the reaction by enclosing the materials in a digester, and then heating the latter for several hours to 100° C. On distilling the crude product thus obtained with water, a large quantity of an ethereal liquid collects upon the surface of the aqueous portion of the distillate. After drying with chloride of calcium, this liquid begins to boil at about 40° C., when a considerable amount of ethylic ether comes over. Afterwards the temperature rises to 70°, between which point and 80° some acetic ether, which had escaped the action of the sodium, distils. The remainder of the distillate, which

was collected apart, came over between 80° and 250°. By repeated rectification, in addition to other products, which belong to another part of the investigation, two liquids were obtained in considerable quantity, one of which boiled at 118°–122°, and the other at about 150°–157° C. On treating these liquids with boiling baryta-water for several hours, the point of ebullition of the first was rendered quite constant at 119°, and that of the second at 151°. Submitted to analysis, the first of these liquids yielded results closely coinciding with those calculated from the formula of butyric ether,

$$C\left\{\begin{array}{l} C_3H_7 \\ O \\ O\ C_2H_5. \end{array}\right.$$

The boiling-point of the new ether also coincides exactly with that of butyric ether, as does also its vapour-density, which was found to be 3·96, the vapour-density of butyric ether being 4·04. Its density in the liquid state is ·8942 at 0° C., that of butyric ether being ·9019 at 0° C. The synthesized butyric ether is readily decomposed by alcoholic potash, yielding alcohol and a salt which, when distilled with excess of sulphuric acid, gives a powerfully acid oily liquid, tolerably soluble in water, possessing in a high degree the characteristic odour of butyric acid, and boiling fixedly at 161° C. The boiling-point of butyric acid has been variously stated by different observers: Pelouze and Gélis give it as 164°, whilst H. Kopp makes it 157°, at 760 millims. pressure. This acid gave numbers, on analysis, exactly corresponding with the formula

$$C\left\{\begin{array}{l} C_3H_7 \\ O \\ O\ H. \end{array}\right.$$

Boiled with water and silver carbonate, it yields, after some hours, a crop of beautiful ramiform needle-like crystals, aggregated into large globular masses, which become anhydrous *in vacuo*; both the mother-liquor and crystals have a faint smell of rancid butter. Submitted to analysis they yielded results closely corresponding with those required for butyrate of silver,

$$C\left\{\begin{array}{l} C_3H_7 \\ O \\ O\ Ag. \end{array}\right.$$

We reserve for a future communication the decision of the question as to whether the butyric acid thus obtained is identical with that produced by the process of fermentation; but we may now state that the synthesized butyric ether possesses, in a very dilute form, a fruity smell, but differing in this respect somewhat from that of the butyric ether ordinarily sold as essence of pine-apples. We have also reproduced the ether from the baryta-salt with the same result as regards odour.

The production of butyric from acetic ether, by the consecutive action of sodium and iodide of ethyl, is expressed by the following equations:—

$$
2\mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{H}\\\mathrm{H}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}\end{array}\right. + \mathrm{Na_2} = 2\mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{Na}\\\mathrm{H}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}.\end{array}\right. + \mathrm{H_2}
$$

Acetic ether. Sodacetic ether.

$$
\mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{Na}\\\mathrm{H}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}\end{array}\right. + \left.\begin{array}{c}\mathrm{C_2H_5}\\\mathrm{I}\end{array}\right\} = \mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{C_2H_5}\\\mathrm{H}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}.\end{array}\right. + \mathrm{NaI}
$$

Sodacetic ether. Butyric, or ethacetic ether.

It has been already stated above that an acid of the same composition as butyric acid must also result from the replacement of two atoms of hydrogen, in the methyl of acetic ether, by two of methyl; and we have in fact produced this acid by first replacing the two atoms of hydrogen by sodium, and then acting upon this compound with iodide of methyl:—

$$
\mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{H}\\\mathrm{H}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}\end{array}\right. + \mathrm{Na_2} = \mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{Na}\\\mathrm{Na}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}\end{array}\right. + \mathrm{H_2}.
$$

Acetic ether. Disodacetic ether.

$$
\mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{Na}\\\mathrm{Na}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}\end{array}\right. + 2\left.\begin{array}{c}\mathrm{CH_3}\\\mathrm{I}\end{array}\right\} = \mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{CH_3}\\\mathrm{CH_3}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,C_2H_5}\end{array}\right. + 2\,\mathrm{NaI}
$$

Disodacetic ether. Butyric, or dimethacetic ether.

We have not yet obtained this ether in a state of perfect purity; but by acting upon the crude product of the reaction with alcoholic potash, a mixture of potassium acetate and butyrate was obtained, and yielded, by the application of Liebig's admirable method of partial saturation, butyric acid in a state of such purity that a further semisaturation produced no change in its composition. A barium salt and a silver salt made from this acid yielded results on analysis closely corresponding with the formulæ

$$
\mathrm{C_2}\left\{\begin{array}{l}2\mathrm{C}\left\{\begin{array}{l}\mathrm{CH_3}\\\mathrm{CH_3}\\\mathrm{H}\end{array}\right.\\\mathrm{O_2}\\\left.\begin{array}{l}\mathrm{O}\\\mathrm{O}\end{array}\right.\mathrm{Ba''}\end{array}\right. \qquad \mathrm{C}\left\{\begin{array}{l}\mathrm{C}\left\{\begin{array}{l}\mathrm{CH_3}\\\mathrm{CH_3}\\\mathrm{H}\end{array}\right.\\\mathrm{O}\\\mathrm{O\,Ag}\end{array}\right.
$$

Barium butyrate, or dimethacetate. Silver butyrate, or dimethacetate.

It is thus evident that an acid, having the composition of butyric acid, can be now produced by three distinct synthetical processes, viz. 1st, by

the introduction of propyl into carbonic acid; 2ndly, by the substitution of ethyl for hydrogen in acetic ether; and 3rdly, by the replacement of hydrogen by methyl in acetic ether. The ethers of these acids may be thus represented:—

$$C\left\{\begin{array}{l}C_3H_7\\O\\O\,C_2H_5\end{array}\right. \qquad C\left\{\begin{array}{l}C\left\{\begin{array}{l}C_2H_5\\H\\H\end{array}\right.\\O\\O\,C_2H_5\end{array}\right. \qquad C\left\{\begin{array}{l}C\left\{\begin{array}{l}CH_3\\CH_3\\H\end{array}\right.\\O\\O\,C_2H_5\end{array}\right.$$

Propyl-carbonic ether. Ethacetic ether. Dimethacetic ether.

Are these acids identical, or are they isomeric? We hope shortly to be able to answer this question decisively.

Synthesis of Caproic Ether.

The production of a dimethacetic compound, as above described, obviously points out a reaction by which caproic or diethacetic ether can be obtained. It is only necessary to act upon disodacetic ether with iodide of ethyl, to obtain, with the greatest facility, the compound in question:—

$$C\left\{\begin{array}{l}C\left\{\begin{array}{l}Na\\Na\\H\end{array}\right.\\O\\O\,C_2H_5\end{array}\right. + 2\left.\begin{array}{l}C_2H_5\\I\end{array}\right\} = C\left\{\begin{array}{l}C\left\{\begin{array}{l}C_2H_5\\C_2H_5\\H\end{array}\right.\\O\\O\,C_2H_5\end{array}\right. + 2\,Na\,I$$

Disodacetic ether. Diethacetic or caproic ether.

Diethacetic ether boils constantly at 151° C. The boiling-point of ordinary caproic ether is stated by Lerch to be 120°, and by Fehling 162°. These numbers differ so widely that it is impossible to use them for comparison. Its specific gravity at 0° C. is ·8822 (according to Fehling the density of caproic ether is ·882 at 18° C.), and its vapour-density 5·00, the theoretical number being 4·98. On analysis it yielded numbers corresponding with the above formula. Diethacetic ether possesses a peculiar and somewhat pleasant odour, somewhat resembling oil of peppermint; it is insoluble in water, but miscible in all proportions with alcohol and ether. Treated with alcoholic potash it is readily decomposed, yielding alcohol and potassium diethacetate, and by distilling the latter with dilute sulphuric acid, diethacetic acid distils over and floats on the surface of the water which accompanies it. This acid reddens litmus-paper powerfully, is very sparingly soluble in water, and emits a peculiar odour, quite different from that of ordinary caproic acid. Boiled with water and carbonate of silver it yields, on filtration and evaporation *in vacuo*, splendid fern-like crystals, which, after pressing between folds of blotting-paper and drying *in vacuo*, with the exclusion of light, are perfectly white, with a satiny lustre; they possess great elasticity, and are remarkably like asbestos. In a strong light they rapidly become brown. Sub-

mitted to analysis this salt exhibited the composition required by the formula

$$C\left\{\begin{array}{l} C\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ H \end{array}\right. \\ O \\ O\,Ag. \end{array}\right.$$

Diethacetate of silver differs from the silver salt of the caproic acid prepared from cyanide of amyl, by its much greater solubility in water, and by its ramiform crystallization, amylic caproate of silver crystallizing in large and very thin plates, which are nearly insoluble in cold water.

In conclusion, there can be no doubt that this reaction is capable of a very wide extension, and that, by its means, we shall be able to ascend many of the well-recognized homologous series. Whilst pursuing it in the acetic and benzoic series of ethereal salts, we also purpose to extend it to the alcohols and ethers.

May 4, 1865.

Major-General SABINE, President, in the Chair.

In compliance with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows :—

Henry Christy, Esq.
The Hon. James Cockle, M.A.
Rev. William Rutter Dawes.
Archibald Geikie, Esq.
George Gore, Esq.
Robert Grant, Esq., M.A.
George Robert Gray, Esq.
George Harley, M.D.
William Huggins, Esq.
Sir F. Leopold McClintock, Capt. R.N.
Robert McDonnell, M.D.
William Kitchen Parker, Esq.
Alfred Tennyson, Esq., D.C.L.
George Henry Kendrick Thwaites, Esq.
Lieut.-Col. James Thomas Walker, R.E.

David Livingstone, LL.D., and the Right Honourable Lord Dufferin were admitted into the Society.

The following communications were read :—

I. "On the Properties of Liquefied Hydrochloric Acid Gas." By GEORGE GORE, Esq. Communicated by Professor STOKES, Sec. R.S. Received March 30, 1865.

In a former communication to the Royal Society "On the Properties of Liquefied Carbonic Acid," printed in the Philosophical Transactions for 1861 (also in the Journal of the Chemical Society, vol. xv., page 163)*,

* The reader is referred to the above communication for details of information respecting the apparatus employed and manipulation adopted.

I described a mode of manipulation whereby various solid substances were introduced into that liquefied gas whilst under very great pressures (varying from 500 to 1100 pounds per square inch), and the action of the liquid upon them observed.

The experiments described in the present paper were made in a similar manner, but with some improvements in safety of manipulation, and in the mode of discharging the tubes, so as to recover the immersed solids in a satisfactory state.

The glass tubes in which the gas was condensed were about $\frac{3}{16}$ths of an inch internal diameter, and fully $\frac{3}{8}$ths of an inch external diameter. Each tube was, before bending, $11\frac{1}{4}$ inches long; it was bent, at $1\frac{1}{4}$ inch and $6\frac{1}{4}$ inches respectively from one end, to the form already described in the paper referred to, thus giving 5 inches in length for the salt, 5 inches for the acid, and $1\frac{1}{4}$ inch for the liquefied gas. These distances are essential; for if the quantities of acid and salt are not properly proportioned to each other, and to the remaining space in the tube, the liquefied product will be very small. The curve in the tube between the acid and the salt should be very gradual, and the other bend much less so. The end of the tube containing the salt should be constructed open, with a flange, and be closed securely by a plug of gutta percha in the same manner as the upper end.

The materials used were strong sulphuric acid and fragments of sal-ammoniac. Each tube was placed in a deal frame or box 10 inches high, 8 inches wide, and 4 inches from front to back, open at the back, and with a front or door of wire gauze. The tube was supported by a cork fitting into a hole in the side of the frame, and was secured within a notch in the cork by a ligature of wire. By means of this arrangement the acid and salt were brought into mutual contact by turning the box itself, without incurring the danger of putting one's hand inside the box and turning the tube alone, as in the former experiments.

The annexed figures (1 & 2) represent the position of the box, 1st, when charged and ready for the decomposition of the sal-ammoniac; and 2nd, after the decomposition is completed. The arrows indicate the direction in which the box is turned.

The action at first should be very slow; otherwise the bubbles of gas will convey the sulphuric acid into the short end of the tube, and endanger the purity of the liquefied hydrochloric acid. The action of the acid was less violent than when generating carbonic acid, and the process was less frequently stopped by clogging of the tube. The liquefied gas was condensed in contact with the various solid bodies by application (from behind) of cotton wool, wetted with ether, to the short end of the tube, as in the former experiments.

Each tube was discharged of its contents by taking hold of it with an ordinary wooden screw clamp support, and immersing its lower end in a vessel of nearly boiling water behind a protecting screen. The explosion

quickly occurred, generally without fracture of the tube, and the substances operated upon could in nearly all cases be readily extracted for examination without suffering injury by coming into contact with the saline contents of the tube. Powdered substances, however, were frequently lost during the discharge, owing to the sudden expansion of the gas in their pores expelling them from the small glass cup. The great degree of pressure (probably about 700 pounds per square inch and upwards) to which the various substances were subjected, frequently made them very hard.

Fig. 1.

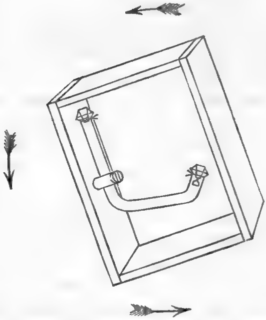
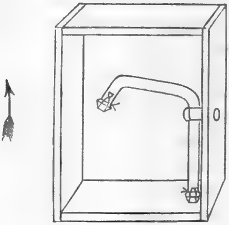


Fig. 2.



The chief inconvenience met with in these experiments arose from the action of the liquefied acid upon the upper gutta-percha stopper, causing the acid to become dark red-brown and opaque, and preventing accurate observation of the substances—also, on discharge of the tube, causing the glass cup and its contents to become coated with a tenacious film of gutta percha. To obviate this inconvenience as much as possible, the inner end of the upper stopper was carefully coated with melted paraffin.

During the early part of each experiment, the liquefied acid was repeatedly poured back, and redistilled by the application of ether, in order to free it from colour imparted to it by the stopper, and also to make its solvent or other action upon the immersed body more rapid. The action of the liquid acid upon the bodies was only continued a few days; and in many cases the acid was not in a liquid state the whole of the time, but only at intervals; in all cases, however, the period of immersion was abundantly sufficient for the liquefied acid to produce its full effect.

The effects in nearly all cases were of so distinct a character, and the conditions under which they were produced so definite, as not to require repetitions of the experiments; but those which were in any respect uncertain were repeated, and those also which were of an important or striking character were likewise repeated, in order to remove the least shadow of a doubt that might be raised respecting them.

The liquid acid is a very feeble conductor of electricity. Two fine platinum wires, immersed in it $\frac{3}{8}$ths of an inch in length and $\frac{1}{10}$th of an inch asunder, and connected with a series of 10 Smee's elements, evolved no perceptible bubbles of gas, and produced only a moderate deflection (amounting to 23 degrees) of the needles of a sensitive galvanometer; and this amount of conduction might possibly have been due to a minute trace of oil of vitriol mixed with the liquid acid. In a second similar experiment, with the wires $\frac{1}{16}$th of an inch apart, not the slightest conduction occurred on using the same battery-power, but by employing the secondary current of a strong induction-coil with condenser attached, conduction and a steady deflection of the needles of the galvanometer (26 degrees) took place, gas being freely evolved from the negative wire only. On separating the brass points of the secondary terminals beyond the distance of the thickness of a thin address card, sparks ceased to pass between those points, and gas was evolved copiously in the liquid acid, apparently in the *mass* of the acid between the two platinum wires as well as at the wires themselves; two similar platinum wires in dilute hydrochloric acid in the same circuit evolved very little gas. It is probable that much of the gas evolved in the liquefied acid was not a product of electrolysis, but simply the acid itself volatilized by the thermic or other action of the current. No sparks occurred at any time in the liquid acid. It is evident therefore that liquefied hydrochloric acid gas is a very bad conductor of electricity, but it is not nearly so powerful an insulator as liquefied carbonic acid gas.

The following experiments illustrate its chemical, solvent, or other action upon various substances immersed in it. The quantity of the solid substances employed was in nearly all cases very small in proportion to that of the liquid acid in contact with them, and in many cases did not amount to one-twentieth of its volume.

A piece of charcoal remained unchanged at the end of ten days, the acid being in a liquid state in contact with it at intervals. A fragment of fused boracic acid did not lessen in bulk or alter in appearance in seven days. White phosphorus was undissolved and unchanged in nine days, and remained equally inflammable. A fragment of ordinary sulphur did not dissolve or alter in several days. Fragments of vitreous black selenium did not dissolve or change in six days. Iodine dissolved rather freely, and quickly formed a purple-red solution. A piece of pentachloride of phosphorus softened in the gaseous acid, and dissolved quickly and completely in the liquid acid, forming a colourless solution. A fragment of sesquicarbonate of ammonia swelled and became full of fissures in the gaseous acid, but neither evolved gas nor dissolved when the liquid acid came into contact with it; after three days' intermittent immersion in the liquid acid, the saline residue evolved no gas on immersion in dilute hydrochloric acid. A piece of sal-ammoniac, immersed almost constantly during nine days, remained undissolved and unchanged.

Potassium evolved no gas when the liquid acid came into contact with it; after eight days it was sometimes enlarged in bulk, and from the outset it was of a white colour; it did not at all dissolve. In a second experiment the results were precisely similar; after three days' intermittent immersion the saline residue showed no signs of containing free potassium on immersing it in dilute hydrochloric acid. Anhydrous carbonate of potash in powder evolved no gas on first coming into contact with the liquid acid; after three days' occasional immersion it remained undissolved, and the residue evolved no carbonic acid on immersion in dilute hydrochloric acid. A crystal of chloride of potassium did not dissolve or change in appearance by four hours' immersion in the liquefied acid. Powdered chlorate of potash imparted a yellow colour to the liquid acid, and did not lessen in bulk during three days' constant immersion; the upper gutta-percha stopper became quite white at its inner end. A crystal of nitrate of potash became of a brownish colour before the gas liquefied, and remained undissolved after six days' intermittent immersion; the upper gutta-percha stopper was unusully acted upon, and of a nankeen colour.

Sodium became white and swelled largely before the gas liquefied. No visible gas was evolved by it in the liquid acid. After three days' intermittent immersion the residue contained no sodium in the metallic state, and no portion of it imparted a blue colour to damp litmus paper. Anhydrous carbonate of soda in powder immersed one hour and a quarter in the liquid acid evolved no visible bubbles of gas, and lost its alkaline reaction (with litmus paper) to about three-fourths of its depth. A fragment of fused sulphide of sodium produced a slight sublimate of a yellowish-white colour in the gaseous acid, and turned of a yellowish-white colour. It evolved no visible gas in the liquefied acid*. After three days' variable immersion it was of a yellowish-white colour, and somewhat enlarged in bulk; the residue evolved no sulphuretted hydrogen by immersion in dilute hydrochloric acid, and its solution gave a perfectly white precipitate with acetate of lead, and imparted no dark colour to sulphate of copper.

Precipitated carbonate of baryta in powder evolved no visible gas by immersion in the liquid acid; it remained undissolved and unchanged in appearance during three days' immersion; the residue evolved a minute quantity of gas by contact with dilute hydrochloric acid. Precipitated carbonate of strontia in powder behaved like carbonate of baryta; the residue, after three days' immersion, was lost during the discharge. A minute fragment of anhydrous Bristol lime exhibited no solution or alteration by nearly constant immersion during eight days in the liquid acid. On removal from the tube, it imparted a strong blue colour to neutral litmus paper by slight friction. On fracture it was found similarly alka-

* Probably the sulphuretted hydrogen set free was in a liquid state, and therefore no bubbles of gas appeared. I found by experiment that hydrochloric acid and hydrosulphuric acid, generated together and condensed into a liquid state, did not form two separate strata of liquid.

line throughout, and exhibited a slight change of colour, extending from its surface to the centre, as if the gas or liquid had been forced into its pores. In a second experiment of three days' intermittent immersion, precisely similar effects were obtained. Several minute fragments of very soft marble were immersed in the liquid acid at intervals during seven days. No gas was evolved when the liquid touched them. On removal from the acid, their physical characters appeared unaltered; they were insoluble in water, but quickly dissolved in dilute hydrochloric acid, with copious evolution of gas. A fragment of bone-earth did not dissolve or alter in appearance during seven days.

Bright magnesium ribbon slowly became dull in the liquid acid, without visible evolution of gas; after seven days' intermittent immersion it was still (with the exception of a thin film) in the metallic state. In a second experiment of three days' constant immersion, similar effects occurred; the residue dissolved and floated in dilute sulphuric acid, with copious evolution of gas. A wire of magnesium and one of platinum immersed in the liquid acid, and connected with a sensitive galvanometer, evolved no perceptible electric current, and only a barely perceptible current after two days of constant immersion. Calcined magnesia in powder did not dissolve or alter in appearance during four days' nearly constant immersion. Oxide of cerium (containing some oxide of didymium and lanthanum) remained undissolved and unchanged in colour during nine days; the residue was insoluble in water. Metallic aluminium became dull in the gas, and quickly dissolved, with evolution of gas, when the liquid acid came into contact with it, and formed a colourless solution. A wire of aluminium and one of platinum, immersed $\frac{1}{8}$th of an inch apart in the liquefied acid, and connected with a sensitive galvanometer, produced a steady deflection of $12\frac{1}{2}$ degrees, the aluminium being positive; the deflection gradually increased to 17 degrees in one hour, and two layers of liquid formed, the lower one brown in colour, and the upper one nearly colourless. The conductivity of the liquid acid was probably increased by the metallic aluminium dissolved in it. Precipitated alumina did not visibly alter or dissolve during six days; the residue deliquesced in damp air. Precipitated silica in powder did not dissolve or visibly alter during four days. Precipitated titanic acid in powder (pale flesh-colour) slightly dissolved in seven days.

A fragment of fused tungstate of soda did not alter in bulk during ten days; it had then acquired a superficial green colour. Molybdic acid in powder turned dark green, but remained undissolved at the end of nine days. Native sulphide of molybdenum remained undissolved and apparently unchanged during two days. Molybdate of ammonia in powder became yellowish green in the gas; it became grass-green in colour in the liquefied acid, but did not dissolve in four days. Sesquioxide of chromium in powder did not dissolve in six days, but became of a dull blackish-brown colour. A fragment of anhydrous yellow chromate of potash became

red before the gas liquefied, but did not dissolve or otherwise alter in the liquid acid. Sesquioxide of uranium became of a paler yellow colour in the gas, but did not dissolve in the liquid acid in six days; the residue was entirely soluble in water. Precipitated black oxide of manganese in powder, and free from water, became quite white in the gas; it remained white in the liquid acid without evolving visible bubbles of gas, and did not lessen in bulk in seven days. A crystal of permanganate of potash softened and swelled in the liquid acid, but did not dissolve in five days; it remained of a dark colour; the residue placed in distilled water produced no coloration.

A crystal of metallic arsenic remained perfectly bright and unchanged in bulk during three days' immersion. Arsenious acid in powder quickly liquefied in the gas, and dissolved to a colourless solution in the liquid acid. A crystal of arsenic acid softened before the gas liquefied, and dissolved quickly and freely in the liquid acid to a colourless solution. Bisulphide of arsenic in powder did not dissolve in six days, but became slightly less red and more yellow; a slight yellowish-white sublimate occurred in the tube during the generation of the gas. Teriodide of arsenic in powder slightly dissolved to a purple-red liquid; apparently only a trace of its iodine was extracted, as its bulk was not visibly less in three days. A crystal of bright antimony remained perfectly bright and unchanged after nine days' intermittent immersion. Precipitated teroxide of antimony became partly liquid before the gas liquefied; it dissolved in the liquid acid quickly and rather freely, and made a colourless solution. A fragment of precipitated antimonic acid did not dissolve in six days. A fragment of black tersulphide of antimony evolved a film of yellowish-white sublimate, and lessened in bulk before the gas liquefied; it decomposed and dissolved in the liquid acid in about a quarter of an hour, and formed a colourless solution which exhibited no further change during seven days. A fragment of bright metallic bismuth remained undissolved and unchanged in the liquid during three days.

Bright zinc evolved no visible gas in the liquid acid, and was not perceptibly corroded in three days. Oxide of zinc slowly dissolved in seven days. Metallic cadmium evolved no gas in the liquid, and was not sensibly corroded in three days. Precipitated carbonate of cadmium evolved no visible gas in the liquid acid, and remained undissolved and unchanged in appearance during seven days. Yellow sulphide of cadmium evolved a trace of white sublimate before the gas liquefied; in the liquid acid it became quite white, and remained undissolved in seven days; on removal it was hard in texture and quite white throughout, and evolved no odour of sulphuretted hydrogen or separation of sulphur on treatment with strong nitric acid. Bright tin evolved no visible gas in the liquid acid; after ten days' intermittent immersion it was converted, to some depth of its substance, into a bulky white solid with deep fissures. In a second experiment of three days' immersion, similar results occurred; all the tin was corroded except a minute fibre in the centre, the white solid was imper-

fectly soluble in water, but instantly soluble in dilute hydrochloric acid. Binoxide of tin in powder did not dissolve in seven days; the residue was white and insoluble in water. A crystal of protochloride of tin softened before the gas liquefied, and partly dissolved in the liquid acid in four days. Bright metallic thallium evolved no gas in the liquid acid, and was only superficially blackened without further corrosion after three days' immersion. Metallic lead did not evolve visible gas in the liquefied acid; it became blackened at first, and in ten days was corroded deeply to a white substance. Red oxide of lead quickly became white in the liquid acid, but did not dissolve in seven days; it was then quite hard, white throughout, and not readily soluble in water. Precipitated carbonate of lead evolved no visible gas in the liquid acid, and remained undissolved after three days' immersion; the residue evolved no gas by contact with dilute hydrochloric acid. Precipitated sulphide of lead in powder produced a faint film of white sublimate in the gas, and by a few hours' immersion in the liquid acid became wholly white; it did not not dissolve during seven days, and was then quite white throughout, and not readily soluble in water. Yellow iodide of lead did not dissolve in seven days, but became of a purplish brick-brown colour and evolved a strong odour of free iodine; it produced yellowish-brown stains upon paper. Yellow chromate of lead evolved at first (in the gaseous acid) a small quantity of deep-red vapour, which condensed as a red moisture near it on the tube; the chromate became white in the gas, and did not dissolve in the liquid acid in three days; it was then a soft white solid, not freely soluble in water, and imparted a faint greenish tint to water.

A minute fragment of iron remained bright, and evolved no gas when the liquid acid came into contact with it; after nine days of intermittent immersion it was only slightly tarnished, and on removal from the acid was found otherwise unaltered. A fragment of fused sulphide of iron produced a faint film of whitish sublimate at first, but evolved no bubbles of gas on contact with the liquid acid; it did not dissolve or alter in appearance. A second fragment constantly immersed during three days behaved similarly; it was as hard as before immersion, and evolved sulphuretted hydrogen freely in hot dilute sulphuric acid. A crystal of green vitriol became yellowish white and opaque in the liquid acid, but did not diminish in volume in six days; the residue was a soft opaque yellowish-white solid. Oxide of cobalt in powder exhibited no change or solution during three days; on removal it was found to be very hard, of a light-brown colour, and dissolved in water, producing a pink solution with separation of black oxide. Peach-coloured carbonate of cobalt evolved no visible gas in the liquid acid; it became greenish blue, but did not lessen in bulk in three days; the residue became pink in the air, and dissolved almost completely in water, forming a pink liquid; it also dissolved in dilute hydroclhoric acid without evolving bubbles of gas. Anhydrous chloride of nickel did not dissolve in the liquid acid in six days. Metallic copper soon lost its brightness

in the gas; it evolved no gas in the liquid acid, and was only slightly corroded after seven days. Black oxide of copper became of a lighter colour in the liquid acid, but did not lessen in bulk in seven days; the residue was a greenish and yellowish white powder, which instantly turned black in water, forming a pale-blue solution, and left black oxide of copper. A crystal of blue vitriol became of a light brown colour in the liquid acid, but did not dissolve in six days; on removal it was found to be a brown soft solid. Protoxide of mercury became white in the gas, and did not dissolve by constant immersion in the liquid acid in four days; the residue was a white solid, soluble in water. Vermilion in powder slowly changed in the liquid acid in three days to a pinkish-white solid, but did not dissolve. Scarlet iodide of mercury in powder imparted a red colour to the liquid acid, but did not lessen in bulk or change in colour during three days; the residue lost its red colour on the application of heat. A fragment of protochloride of mercury did not visibly alter in the liquid acid in four days. Metallic silver did not dissolve or become much corroded during seven days. Oxide of silver became white in the liquid acid in one day, but did not dissolve. Precipitated chloride of silver in powder did not visibly alter or dissolve during sixteen days. Metallic platinum was unaffected in the liquid acid.

Oxalic acid was slightly dissolved in the liquid acid in three days without change of colour. Uric acid remained undissolved and unchanged during three days. Paraffin did not appear to be dissolved or affected in nine days. Gutta percha was quickly acted upon; it imparted to the liquid acid, first a red, and ultimately a dark-brown colour; it appeared also to dissolve in the acid to some extent, and on discharging the tubes was left behind as a tenacious coating upon the adjacent parts. Gun-cotton was unaffected in the liquid acid. Cotton was not visibly altered in two days. Solid extract of litmus dissolved slightly, forming a faintly purple blue or inky solution; it became of a dark red colour and enlarged in bulk; the residue formed a perfect solution in water; the solution was red in colour.

Remarks.—The foregoing experiments show that liquid hydrochloric acid has but a feeble solvent power for solid bodies in general. Out of 86 solids it dissolved only 12, and some of those only in a minute degree; of 5 metalloids it dissolved 1, viz. iodine; of 15 metals it dissolved only 1, viz. aluminium; of 22 oxides it dissolved 5, viz. titanic acid, arsenious acid, arsenic acid, teroxide of antimony, and oxide of zinc; of 9 carbonates it dissolved none; of 8 sulphides it dissolved 1, viz. tersulphide of antimony; of 7 chlorides it dissolved 2, viz. pentachloride of phosphorus and protochloride of tin; and of 7 organic bodies it dissolved 2.

The results show also that liquid hydrochloric acid in the anhydrous state manifests much less chemical action upon solid bodies than the same acid when mixed with water as under ordinary circumstances; for instance, the difference of its action upon magnesium, zinc, cadmium, and even aluminium, under the two conditions, is very conspicuous. This may

arise in a great measure from its feeble solvent capacity—insoluble films forming upon the surface of the bodies immersed in it preventing its continued contact and further action. This want of contact could hardly have been the case in the remarkable instance of caustic lime: here was a powerful and true acid (*i. e.* a hydrogen acid) and a powerful base; each in a nearly pure state; both possessing under ordinary circumstances a very powerful chemical affinity for each other; the one a liquid, and the other a porous solid; brought into intimate contact by an enormous pressure forcing the liquid into the porous solid; the solid base being very small in bulk, and the liquid acid largely in excess, probably fifty times the quantity necessary for its saturation; and the action extended over a far greater period of time than would in the presence of water been at all necessary: nevertheless no perceptible chemical action occurred; the two remained totally uncombined.

It must not be overlooked that the results are partly due to anhydrous hydrochloric acid in the *liquid* state, and partly to the same acid in the *gaseous* state, under great pressure, the one class of effects not being eliminated from the other in the present experiments; it is probable that if the substances could have been submitted to the action of the *liquid* acid *alone*, the chemical effects would have been much smaller even than they were. For instance, the action upon potassium, sodium, and tin appeared to be due to the influence of the acid in the gaseous state, as no gas was perceptibly evolved by these metals in the liquid acid. In the cases of potassium and sodium (the latter in particular) it is perhaps possible, though highly improbable, that the whole of the metal had been corroded before the liquid acid touched it; but with tin this was certainly not the case, some metallic tin being left uncorroded at the end of the experiment.

Oxides in general, with the exception of lime and certain others which do not readily combine with aqueous hydrochloric acid, were slowly converted in a greater or less degree into chlorides. Carbonates also, except that of lime, were in general converted in a greater or less degree into chlorides.

Such carbonates as were decomposed evolved no visible bubbles of gas in the liquid acid: this may be explained on the supposition that they were previously completely decomposed by the *gaseous* acid during the process of generation (this, however, was not the case with carbonate of soda), or that the liberated carbonic acid was in the *liquid* state and was *dissolved* by the liquid hydrochloric acid. In my former paper it was shown that liquid carbonic and hydrochloric acids generated and condensed together did not form two separate strata of liquid.

Sulphides were in some cases converted into chlorides; in other cases not so; in nearly all cases a trace of whitish sublimate was produced in the *gaseous* acid. The chlorate and nitrate of potash were both decomposed.

I may here take the opportunity of stating that tubes charged with liquid carbonic acid in October 1860 suffered no leakage by February 1865.

II. "On the Production of the so-called 'Acute Cestode Tuberculosis' by the administration of the Proglottides of *Tænia mediocanellata.*" By JAMES BEART SIMONDS, Esq., Professor of Cattle-Pathology in the Royal Veterinary College, and T. SPENCER COBBOLD, M.D., F.R.S., F.L.S. Received April 13, 1865.

Neither of us having exhausted certain funds placed at our disposal for scientific purposes (in the one case by the Royal Agricultural Society through the Governors of the Royal Veterinary College, and in the other by the British Association for the Advancement of Science), we have united the resources which severally remained to us, and have instituted a series of experiments in helminthology. These experiments, we are happy to state, have proved, for the most part, eminently successful; moreover, several of them not having been previously performed in this country, we have ventured to think that at least the firstfruits of our combined research in this particular relation might not unfitly be submitted to the notice of the Royal Society.

The subject selected for the experiment which we now proceed to relate, was a fine healthy female calf about a month old, living at the time on the milk of its dam. As we were unable to obtain possession of the dam, another cow was procured as a foster-mother, and the calf was placed with her in order that it might receive a proper supply of milk in the natural way. This plan was preferred to that of obtaining a weaned calf, as being better calculated to preserve the health and strength of the young animal. In the course of a few days the two animals became perfectly accustomed to each other, the calf taking nourishment as often as was requisite.

On the 21st of December, 1864, we administered to the calf eighty mature proglottides of the *Tænia mediocanellata*, mingled with a little warm milk in the form of a draught. The potion was taken readily, and the worm-joints probably entered the stomach in a perfect and unbroken condition. No alteration was made in the subsequent management of the animals, but a careful daily watch was kept upon the calf.

For some time no indications were perceived of disturbed health; but on the 6th of January, 1865 (the sixteenth day after the experiment), a careful observation showed that the animal, although lively (and taking its milk and likewise some hay with undiminished appetite), was nevertheless suffering from some persistent cause of irritation. It would often be nibbling at its legs and other parts of its body, and trying with its mouth and tongue to get at places which were beyond its ordinary reach. It would also frequently rub itself against the manger and sides of the loose box in which it was confined. Desisting from this, it would arch its spine and stretch out its hind limbs in an altogether unusual manner. It would also strain itself repeatedly, at such times voiding either urine or fæces, or occasionally both in small quantity. There was, however, no expression of suffer-

ing in the countenance, no disturbance of the breathing or of the circulation, no injection of the visible mucous membranes, no alteration of the temperature of the body, no "staring" of the coat, nor rigors; in short, no indication of anything seriously wrong. These symptoms continued throughout the next day with little variation; on the third day they had nearly passed away, and by the fourth had entirely disappeared.

On the 25th of January, 1865, just five weeks after the first worm-feeding, two hundred more of the mature proglottides of *Tænia medio-canellata* were administered; but one hundred of these worm-segments had been previously immersed in a weak alcoholic solution, strong enough, it was feared, to destroy the vitality of their contained eggs. The other hundred proglottides were in beautiful condition, and for the most part appeared to be thoroughly mature. Again the calf took the feeding readily, and little or no force had to be employed in holding it during the administration. However, directly on being loosed, it was observed to show some symptoms of distress in the breathing, accompanied with trembling. The feeding took place at 3 P.M., and, as the night promised to be cold, it was placed with the cow in a closed and warm stable. On the following morning it was noticed that the tremors had somewhat abated, but the animal was evidently dispirited, and would every now and then grind its teeth as if in pain. Its appetite was much diminished. By the next day, however, all these diseased symptoms passed away, and the animal recovered its ordinary healthy aspect.

On the 1st of February, the seventh day succeeding the second worm-feeding, there was a decided return of the nervous irritability; but in a day or two these symptoms again declined. Nevertheless the animal was not quite right; the coat began to lose its natural and glossy appearance, and there was an evident loss of flesh.

Feb. 8th.—A marked change for the worse has taken place. The animal is dull and dispirited; refuses all food excepting milk, and of this takes but little; it arches the back frequently, and stretches the limbs in a peculiar manner; the breathing and the pulse have increased, and at intervals slight tremors are observable, more particularly of the muscles of the neck and shoulders.

Feb. 9th.—All the unhealthy symptoms are more marked. The pulse numbers 120, and the breathing 35 in the minute. The tremors are more continuous, and the condition of the animal is fast declining.

Feb. 10th.—Still worse. The calf is so ill that we fear a fatal result. It takes little or no notice of the cow, and cannot be induced to suck. The eyes have a peculiar staring expression.

Feb. 11th.—The severity of the symptoms has somewhat abated this morning. The animal is rather more lively, and will now and then take a little milk. The breathing and pulse, however, remain rapid. The tremors, though still frequent, have diminished in intensity. Towards the after part of the day the improvement became more marked; therefore,

instead of destroying the animal (as we had purposed in the event of its becoming much worse), we resolved to satisfy ourselves, by other means, as to whether the above symptoms were really due to parasite-invasion. Accordingly a small portion of the right sterno-maxillaris muscle was removed by operation, and this fragment of the flesh, although weighing only 22 grains, revealed the presence of three imperfectly developed cysticercus-vesicles. Each was about the size of a pin's head, but they displayed no trace of calcareous corpuscles, or of cephalic formation in their interior. On the assumption (afterwards, however, found to be erroneous) that all the muscles of the body might be similarly affected, and to the same extent, it was at the time calculated that the animal "entertained" some 30,000 of these artificially introduced "guests."

Feb. 12th.—A further improvement has taken place, but the animal is still dispirited, the pulse and breathing continuing abnormally rapid. The eyes are less staring.

Feb. 13th.—Improvement continues; breathing less rapid; the tremors have disappeared.

Feb. 15th.—Pulse diminishing; breathing nearly normal; appetite good.

Feb. 22nd.—Convalescence perfectly re-established.

Throughout the remainder of the month of February, and during the whole of March, the calf continued to maintain complete vigour, and, indeed, gained flesh so rapidly that at the beginning of April it might readily have been sold to a farmer, to a butcher, or to a cattle-dealer, as a thoroughly sound and thriving young beast. The time having, however, arrived for determining the result of the experiment, the calf was slaughtered on the 3rd of April, by division of the right carotid artery. The operation was performed by Mr. Pritchard, who also during the subsequent *post-mortem* examination rendered us essential service. As before, so after its death, all present remarked the particularly healthy aspect of the animal, there being no external indications by which the most practised professional eye could have discovered the existence of internal disease. But for our previous trial we should ourselves have been doubtful of finding any entozoa within the flesh.

Immediately after the first incision along the median line of the thorax, a solitary cysticercus came into view, many others successively displaying themselves as the integument was being raised and dissected from off the left side of the carcass. No person in this country having hitherto witnessed such a demonstration as now followed, we may perhaps be permitted to express the feeling of astonishment which all shared on thus beholding hundreds of larval cestode parasites in the flesh of an animal not usually considered capable of harbouring "measles" after the fashion of swine.

Examined individually, the larvæ were enclosed in oval sacs, whose transparency permitted us to see, at or near the centre of each vesicle, internally, a minute white spot representing the so-called receptaculum capitis.

On subsequent rupture of the cyst, a microscopic examination of the contained larva revealed the ordinary characters of the Cysticercus which produces the *Tænia mediocanellata.*

Speaking generally, it may be said that the connective tissue and cellular aponeuroses were very feebly invaded; but in certain situations, such as those occupied by the linea semicircularis and fascia lumbaris, several vesicles were closely associated; moreover, as regards the muscles themselves, extensive parasitic invasion was prevalent only in the more superficial layers. It was likewise noticed, as obtains in the parallel case of Trichina, that the larvæ were disposed in the longitudinal direction of the muscular fibres, being at the same time more numerously grouped towards the points of osseous insertion or of aponeurotic attachment. Not a few large vesicles had inflamed and suppurated, the cysts being occupied internally by a thick green-coloured deposit.

Referring to the left side only, we noted that all the breast-muscles (pectoralis major, p. transversus, and p. anticus) were much infested, but scarcely so fully as the more superficial panniculus carnosus. In the latissimus dorsi and trapezius the cysts were very numerous, rather less so in the combined levator humeri and sterno-occipitalis, somewhat fewer in the rhomboideus brevis and rhomboideus longus, and exceedingly scanty in the superior part of the scalenus, the remainder of this last-named muscle being entirely free. The lateralis sterni contained none; neither were any observed in the abdominal region of the serratus magnus, but several vesicles were lodged in the superficial cervical portion of this muscle. Not a few existed in the upper part of the complexus major and in the complexus minor, some also occurring in the longissimus dorsi; yet none were observed in the spinalis dorsi, in the superficialis costalis, or in the diaphragm.

Turning towards the neck-region, we found them abundant in the sterno-maxillaris, considerably less so in the splenius, only one in the hyoideus, several in the sterno-hyo-thyroideus, but none in the longus colli. All the other deep-seated muscles of this region, including the obliquus capitis superior and inferior, as well as the rectus capitis posticus major and minor, appeared free from any trace of the vesicles. On the other hand, all the superficial muscles of the face, such as the retractor anguli oris, orbicularis oris, and levator palpebrarum, gave abundant evidence of their presence, the vesicles being particularly numerous at the outer part of the masseter externus. In like manner their presence was only less strongly indicated in the muscles of the eyeball, such as the obliquus inferior, adductor and retractor oculi, also in the depressor oculi, one "measle" being placed between the tendon of this last-named muscle and the sclerotic coat. The ball of the eye itself contained no vesicles. A few were remarked in the substance of the genio-hyoideus and other muscles supplying the tongue; but the lingual organ properly so called appeared to be entirely free.

As regards the anterior extremity, we found the Cysticerci very numerous in the teres externus and abductor humeralis, being scarcely less abundant in the spinatus anticus and posticus. They were likewise prevalent in the front part of the triceps extensor brachii, but altogether wanting behind and in the deeper portions of this muscle. A very few were remarked in the flexor brachii, whilst the subscapularis, teres internus, and coraco-humeralis failed to reveal any. They were very abundant in the flexor metacarpi externus, less so in the flexor metacarpi medius, and comparatively scanty in the flexor metacarpi internus. The lower part of the combined flexor perforatus and perforans showed a few, several being likewise present in the accessorius ulnaris. They were rather more abundant in the extensor metacarpi magnus, also in the extensor et adductor digitorum, likewise in the extensor digiti externus, and scarcely less so in the extensor metacarpi obliquus; yet none could be discovered either in the anconeus or in the humeralis externus.

Over the haunch, and throughout the surface-flesh of the left hinder limb, the Cysticerci were particularly abundant, being numerous in the gluteus maximus, in the tensor vaginæ femoris, and most especially in the large triceps abductor femoris. They were little less abundant in the vastus externus, and in those limited portions of the gastrocnemius externus and internus which come near the surface. A few vesicles were observed at the subcutaneous posterior section of the ischio-tibialis, also in the outer part of the biceps rotator tibialis and rectus femoris; yet none were noticed either in the gluteus internus and gracilis, or in the vastus internus and sartorius. In the flexor metatarsi and extensor pedis they were rather numerous, but, at the same time, comparatively scarce in the peroneus and flexor pedis perforans. Lastly, none were detected in either the psoas magnus or psoas parvus.

With the exception of the heart, none of the viscera showed Cysticerci, the lungs, liver, kidneys, spleen, and thymus gland being absolutely free; neither were any discovered in the brain. In short, it may be stated that the internal organs of the body generally were perfectly healthy; and even as regards the heart itself, the rather numerous vesicles found there displayed only a very incomplete development. At first they looked as if they might belong to a separate swarm-brood; but a careful microscopic examination disproved this notion, and at the same time revealed some curious facts. In the heart none of the vesicles had attained one-third of the size of those prevalent in the muscles, yet their age was doubtless the same; for although none of those examined displayed a well-formed head with the characteristic and normal number of suckers, yet one vesicle was found to possess three suckers, another having two suckers, and a third only a single sucker. Most of the vesicles were entirely suckerless, whilst those which had them showed other indications of abnormality. The suckers themselves were not perfectly formed, in most cases, and there were commencing signs of calcareous degeneration. In some instances, the entire contents of the vesicles

appeared to have been absorbed, leaving only faint white spots to indicate the situations where the cysts once were. Such, at least, is our interpretation of the phenomena observed; and, in this relation, we have only further to remark that the heart-cysts were not merely found at the surface of the organ, but were dispersed throughout its substance, one or two of the better-formed vesicles being lodged within the septum ventriculorum.

On the present occasion we do not propose to offer any lengthened comment on the results of this experiment, but rather to let the facts speak for themselves; nevertheless, to impart an aspect of completeness to our paper, we will offer one or two concluding remarks.

So far as we are aware, only three experiments of this kind have been previously performed on the calf—namely, two by Leuckart, and one by Mosler. In two of these instances the experimental animal perished, whilst in the other case, as in our own, the creature barely escaped with its life. To our animal we administered a larger number of proglottides than had been given even in Mosler's case; but, probably in consequence of the embryonic immaturity of the contents of many of the eggs, we did not get that fatal result which otherwise would inevitably have followed from a larger migration of the cestode-progeny. We believe that by far the greater proportion of the "measles" resulted from the second worm-feeding, in which case they would have come from the hundred proglottides not subjected to the action of alcohol. Although the characters presented by the earlier-developed morbid symptoms, as well as the time of their accession, induce us to attribute the diseased phenomena to the larvæ set free by the first "feeding," yet it is clear, from the feebleness of the symptoms manifested, that only a very inconsiderable number of embryos can have entered on their wanderings. In the second "feeding," however, the case is very different; for here all the circumstances connected with the subsequent and marked disturbance of the animal's health point unequivocally to the development of that peculiar form of parasite-disease which Leuckart has designated as the "acute cestode tuberculosis."

From the number of young vesicles present in the minute portion of muscle removed by operation from the living animal, we had (in the pages of the 'Lancet') publicly announced our belief that we might ultimately find 30,000 Cysticerci developed in this calf; but as the larvæ were subsequently found to be almost entirely confined to the superficial muscular layers, it turned out that our calculation was considerably beyond the mark. Nevertheless from *post-mortem* data we estimate that there were between seven and eight thousand "measles" present, and one of us counted 130 vesicles at the surface of a single muscle.

Lastly, it only remains for us to express our thanks to those gentlemen who supplied us with the necessary experimental material, namely, to Dr. Greenhow for the first tapeworm employed, and to Dr. Anderson and Mr. Brookhouse (Nottingham) for the second and third tapeworms, which were given together at the second administration. Dr. Greenhow's specimen

had the head perfect, both his and Dr. Anderson's examples being quite fresh. Mr. Brookhouse informed us that his specimen had been placed in "very weak spirit"; but it is clear that the worm had been injuriously affected thereby, and the ova had lost their vitality.

III. "On the Rate of Passage of Crystalloids into and out of the Vascular and Non-Vascular Textures of the Body." By HENRY BENCE JONES, A.M., M.D., F.R.S. Received April 26, 1865.

(Abstract.)

The paper is divided into five sections—

1st. On the method of analysis, and its delicacy.

2nd. Experiments on animals to which salts of lithium were given, upon the rate of their passage into the textures.

3rd. On the rate of the passage of lithium-salts out of the textures.

4th. Experiments on healthy persons, and on cases of cataract.

5th. On the presence of lithium in solid and liquid food.

1. Three methods of analysis were followed, according as much or little lithium was present: first, simply touching the substance with a red-hot platinum-wire; secondly, extracting the substance with water; thirdly, incinerating the substance and treating it with sulphuric acid, and exhausting with absolute alcohol. $\frac{1}{12,000,000}$ of a grain of chloride of lithium in distilled water could be detected, and $\frac{1}{6,000,000}$ to $\frac{1}{2,000,000}$ of chloride of lithium in urine.

2. *On Rate of Passage into the Textures through the Stomach.*

Even in a quarter of an hour three grains of chloride of lithium, given on an empty stomach, may diffuse into all the vascular textures, and into the cartilage of the hip-joint and the aqueous humour of the eye. In very young and very small guinea-pigs which have received the same quantity of lithium, in thirty or thirty-two minutes it may be found even in the lens; but in an old pig in this time it will have got no further than the aqueous humour. If the stomach be empty, in an hour the lithium may be very evident in the outer part of the lens, and very faintly traceable in the inner part; but if the stomach be full of food, the lithium does not in an hour reach the lens. Even in two hours and a half lithium may be more marked in the outer than in the inner part of the lens. In four hours the lithium may be in every part of the lens; but less evidence of its presence will be obtained there than from the aqueous humour. In eight hours, even, the centre of the lens may show less than the outer part. In twenty-six hours the diffusion had taken place equally throughout every part of the lens. If the lithium is injected under the skin, in ten minutes it may be found in the crystalline lens, and even in four minutes, after the injection of three grains of chloride, the lithium may be in the bile, urine, and aqueous humour of the eye.

3. *On the Rate of Passage out of the Textures.*

After two grains of chloride of lithium, in six hours the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours no difference in the different parts of the lens was detectable. In forty-eight hours no difference was observed. In ninety-six hours no lithium was detectable in the lens or cartilage of the hip-joint. The urine showed lithium very distinctly even in one drop.

After one grain of chloride of lithium, in five hours and a half the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours and a half there was no difference throughout the lens. In forty-eight hours the watery extract of the lens showed faint traces of lithium. In seventy-two hours and a half (three days) the alcoholic extract of the lens showed no lithium. The urine still showed lithium distinctly in one drop, and it continued to be found in the watery or alcoholic extract for twenty-one days.

After half a grain of chloride of lithium, in three hours and fifty minutes traces of lithium could be found in the lens, and for thirty-seven or thirty-eight days traces of lithium could be found in the urine.

After a quarter of a grain of chloride of lithium, in five hours and a quarter the aqueous humour showed lithium, and all the organs showed lithium, but none was in the lens. In another pig, in twenty-four hours all the organs showed less lithium, and none was found in the aqueous humour.

After a quarter of a grain, in five hours and thirty-five minutes lithium was distinct in the aqueous humour, and very faintly traceable in the lens; and after sixteen days the minutest traces of lithium could be detected in the lens, the liver, the kidney; but no trace could be found in the blood.

After three grains of chloride of lithium, in four hours lithium was in the hair of the belly, and for thirty-two days the urine showed lithium very distinctly. The thirty-third day after the lithium the lens was found to contain minute traces of lithium, and even after thirty-nine days the lithium was in the alcoholic extract of the urine.

With three grains of chloride of lithium, a young pig in half an hour had lithium in the watery extract of the lens. In the same time an old pig had no lithium in the lens.

With two grains, a young pig in six hours had lithium distinctly throughout the whole lens. An old pig in the same time had lithium in the outer part of the lens, but scarcely the minutest trace in the inner part of the lens.

4. *Experiments on Healthy Persons and on Cases of Cataract.*

Ten grains of carbonate of lithia, taken three or four hours after food by a man, require from five to ten minutes to pass from the stomach to the urine, and this quantity of lithia will continue to produce traces of lithium in the urine for from six to seven days.

Two grains of chloride or carbonate of lithia, taken shortly after food by a boy, gives no appearance in the urine until from ten to twenty minutes; and this quantity continues to pass out for five, seven, or eight days.

Experiments made by the ordinary mode of analysis showed that four grains of sulphate of protoxide of iron, taken by a man almost fasting, gave a trace in the urine in seven minutes. Seven grains gave distinct appearance in ten minutes; and in ten minutes and a half one grain of iodide of potassium, taken by the same man fasting, appeared in the urine in twelve minutes.

When no lithia had been taken, seven cataracts were examined most carefully, and only one showed an exceedingly feeble trace of lithium.

When twenty grains of carbonate of lithia were taken twenty-five minutes before the operation, the lens showed no lithium.

When twenty grains of carbonate of lithia were taken two hours and a half before the operation, the lens showed lithium in the watery cataract.

When twenty grains of carbonate of lithia were taken between four and five hours before the operation, the lens showed lithium in each particle.

When twenty grains of carbonate of lithia were taken seven hours before the operation, the lens showed lithium in each particle.

When twenty grains of carbonate of lithia were taken seven days before the operation, the lens showed not the slightest trace of lithium.

Twenty grains of carbonate of lithia, taken between six and thirty-six hours before death, showed the faintest indications of lithium in the lens. The cartilage showed lithium very distinctly.

Ten grains of carbonate of lithia, taken five hours and a half before death, gave only faint traces of lithium in the lens, but the cartilage showed lithium very distinctly.

5. *On the Presence of Lithium in Solid and Liquid Food.*

Potatoes showed traces of lithium once in five trials.
Apples showed traces of lithium thrice in four trials.
Carrots showed no lithium in two trials.
Bread showed traces of lithium thrice in three trials.

Cabbage	,,	,,	twice in two trials.
Tea	,,	,,	eight times in ten trials.
Coffee	,,	,,	four times in five trials.
Port wine	,,	,,	six times in six trials.
Sherry	,,	,,	six times in six trials.
French wine	,,	,,	four times in four trials.
Rhine wine	,,	,,	eight times in eight trials.
Ale	,,	,,	twice in three trials.
Porter	,,	,,	twice in three trials.

Mutton, beef, and sheep's kidney showed no lithium: one kidney had a slight trace.

Conclusions.

1. *On the Rate of Passage of Solutions of Lithium into the Textures of Animals.*

Chloride of lithium taken into the stomach in quantities varying from one quarter of a grain to three grains, will pass into all the vascular parts of the body, and even into the non-vascular textures, in from one quarter of an hour to five hours and a half.

2. *On the Rate of Passage out of the Textures of Animals.*

Chloride of lithium passes out by the skin as well as by the urine; and thus the animals can redose themselves with chloride of lithium from the hair and feet, and prevent accurate observations. Hence probably chloride of lithium, in quantities varying from half a grain to three grains, will continue to pass out of the body for thirty-seven, thirty-eight, or thirty-nine days; and even after thirty-three days, traces may be found in the lens; but in three or four days no lithium may be detectable in the non-vascular textures.

3. In man, carbonate of lithia, when taken in five- or ten-grain doses, may appear in the urine in five to ten minutes if the stomach is empty, or twenty minutes if the stomach is full, and may continue to pass out for six, seven, or eight days.

In two hours and a half, traces may be in the crystalline lens, and in five or seven hours it may be present in every particle of the lens and in the cartilages. In thirty-six hours it may be very evident in the cartilages. And in seven days not the slightest trace may be detectable in the crystalline lens.

4. Though in the solid and liquid food infinitesimal quantities of lithium may enter the body, usually no proof of their presence in the organs or secretions can be obtained.

IV. "Lunar Influence on Temperature." By J. Park Harrison, Esq., M.A. Communicated by the Rev. R. Main, F.R.S. Received April 27, 1865.

The tabulation of an unbroken series of thermometric observations for the several days of the lunation during fifty years having been completed up to November 1864, and an amount of lunar action detected which appears sufficient to set at rest the long vexed question of the moon's influence over our atmosphere, I venture to think that the time has arrived when it becomes a duty to lay the results of the investigation before the Royal Society.

In 1856 the frequent recurrence of higher temperatures about the eighth or ninth day of the moon's age, led to an examination and comparison of the mean temperatures of the third day before, and the second day after first quarter of the moon, for a series of seven years at Chiswick, and sixteen years at Dublin. The results showed conclusively that the temperature of the second day after first quarter was higher than the temperature of the third day before that phase during the years in question.

On extending the investigation to the remaining days of the lunation, the maximum was found to occur, at both stations, at the period when heat was first observed, and the minimum after full moon and last quarter.

The long series of mean temperatures which had been determined by Mr. Glaisher for the British Meteorological Society from observations taken at Greenwich between 1814 and 1856, were next arranged in tables constructed for the purpose. These observations, though corrected by an arbitrary rule totally irrespective of the moon, and in a measure therefore eliminating influences that may have been exerted on the observed temperatures, appeared on the whole the best, as they were also the most extensive printed series existing.

The method pursued.—The Tables were constructed in the following manner:—The mean temperatures of the days on which the moon entered her four principal phases having been first inserted in columns arranged at equal distances, the mean temperatures of the first, second, and third days before and after each of the quarters were entered in the columns adjoining on either side; and any remaining observations in octant columns midway between the quarters*. The deficiency occasionally occurring in an equal number of six observations between the quarters, was supplied by repeating the observation of mean temperature of the third day after, or third day before the quarters, the same observation in such cases being used for both those days. Thus an equal number of observations was secured for twenty-eight days out of 29·5, at all the seasons of the year, a point of no little importance as regards the next process, viz., obtaining true means of the temperatures of the several days. This was done in the usual way, by adding together the observations of mean temperature in each column, and dividing the sums by the number of lunations the temperatures of which had been tabulated.

The last operation consisted in laying down the mean line on scale-paper, and marking above or below it the mean temperatures belonging to the several columns on vertical lines, representing the several days of the lunation preceding or following the four quarters. The points thus marked

* On an average, the number of observations in each of the octant columns equals half the number of observations in the other columns. Their means were not made use of in forming the curves of temperature.

on the scale-paper were united by straight lines, and thus formed what are usually termed "curves" of temperature.

The results of the tabulation of the Greenwich observations.—The tabulation of the mean temperatures of the 520 lunations between 1814 and 1856, resulted in the complete confirmation of the phenomenon originally observed; that is to say, the maximum mean temperature showed itself, as before, in the first half of the lunation, and the minimum mean temperature in the second half of the lunation. The difference between the maximum and minimum temperatures for the 520 lunations was 1° Fahr. (see Pl. V. fig. 1).

In the autumn of 1860 M. Faye communicated the above results to the French Academy*.

Additional Results in 1856–65.—The author has now the honour of laying before the Royal Society additional confirmatory evidence derived from a tabulation of mean temperatures at Greenwich for the eight years, or 99 lunations, which have elapsed since the year 1856†.

Upon examining the lunar curve of temperature derived from these means (see Pl. IV. fig. 2), the maximum mean temperature will be again found in the first half of the lunation, at the moon's first quarter, and the minimum mean temperature in the second half of the lunation. The difference is 3°·5; the maximum is 51°·7; and the minimum 48°·2. The mean of the period is 49°·56.

And on adding the sums of mean temperature of this period to the sums of the mean temperatures in the Table of 520 lunations, and dividing the sums of the several columns by 619 (the number of lunations which occur in fifty years), the maximum is still found to occur at the first quarter, and the minimum shortly after last quarter. The difference beween the maximum and minimum mean temperatures is 1°·33. A curve of the mean temperatures for the 619 lunations will be found in Pl. IV. fig. 1‡.

Explanation of the Phenomenon.—Although the recurrence of higher temperatures in the first half of the lunation, and more particularly at the moon's first quarter—as a meteorological fact—is not affected by the correctness or incorrectness of any explanation which may be given of the phenomenon, yet it will be well to state that a probable cause for the

* Comptes Rendus, December 1860.

† The Tables were laid before the Society, and are available for reference.

‡ As regards the annual sums of temperature of the two days of maximum and minimum, the sums on the former day are higher than the sums on the latter day in 34 years out of 50. And the sum of the differences, in the years in which the mean temperature of the day before first quarter is higher than the mean temperature of the second day after last quarter is 783°·6, whilst the sum of the differences, in the years in which the mean temperature of the former day is lower, is 220°·0. For several years together, however, the day of maximum temperature presents itself, not on the day before first quarter, but a day or two later (see Pl. V. figs. 2, 3, and note *ad fin.*).

apparent paradox of heat occurring at the moon's first quarter suggested itself in 1857*.

It was evident that the effects noticed could not be due to any heat derived directly from the moon. Even if the experiments of Melloni and Bouvard—and, it may be added, the results obtained by Professor Piazzi Smythe on Teneriffe—had not established it as a fact that no serviceable heat, dark or luminous, reaches the lower strata of the earth's atmosphere at the period of full moon, the results of the tabulation of mean temperatures at various stations and for different periods of time show that, with some remarkable exceptions to be hereafter accounted for, cold displays itself on the average in the *second* half of the lunation, and a higher temperature at first quarter—at the very time when it may be supposed that the moon has parted with the whole of the heat she has received from the sun, and her crust opposite the earth has not been subjected to the solar rays for a sufficiently long period for lunar radiant heat to exercise any thermal action, either direct or indirect, on our atmosphere.

This being so, the concurrent results of investigations undertaken by eminent physicists in this and other countries point to a maximum of cloud, rain, and vapour-bearing winds in the first half of the lunation, when the curves indicate heat †; and a minimum of cloud and rain, with drier winds, in the second half of the lunation. It was not difficult then to connect the two phenomena—all gardeners being practically aware of the fact that heat is retained in the soil by the agency of cloud ‡. Professor Tyndall has shown by his elaborate experiments, that this is the case also with respect to the aqueous vapour of the atmosphere.

Whether the dispersion of Cloud is due to the Radiant heat of the Moon.—As regards the degree of heat which is attained by the moon, Sir John Herschel estimates it as equal to the boiling-point of water; and the same eminent person considers that the radiation of this heat would be sufficient to disperse cloud in the upper regions of the air.

The estimate of the moon's heat appears to be that of our satellite at the period of opposition. But the maximum heat would not be attained until several days later; for, the moon always turning the same face to the earth, her crust directly opposite to us does not attain its greatest heat until last quarter, at which time not only will it have received the sun's rays for twice the number of days during which that surface had been heated at the time of opposition, but the adjoining region also (eastward of it), itself recently illuminated and heated for fourteen, thirteen, and twelve times the length of our day of twenty-four hours, although the sun's

* See Brit. Assoc. Reports, 1857, p. 248.

† The number of clear and cloudy days at Greenwich, during the seven years (1841–47) that bihourly observations were made at that station, also corresponds with the hot and cold periods at the station.

‡ See also Mr. Glaisher's paper on the subject in the Philosophical Transactions.

rays have passed from it, still radiates the heat that has been absorbed, and which it may be presumed has penetrated to a depth (according to the speed with which the moon is travelling) commensurate with the time of its exposure to the sun.

Again, as regards the date of the minimum temperature of the moon, doubtless the absence of all atmosphere must greatly augment the action of lunar radiation; yet it is impossible to believe that the flood of heat poured upon the moon day and night for so many days together, without intermission, can be speedily dissipated. It would be more consistent with the analogy of terrestrial meteorology that the state of cold in the moon should be prolonged beyond the renewal of the sun's radiation, and consequently no heat from her crust reach the limits of our atmosphere at first quarter.

It would be strictly according to analogy, also, if the length of time which the moon's surface-crust takes to attain its maximum heat were found to be greater than that which it takes in falling to its minimum. Now there appears some reason to believe that this is the case; and as the mean temperature of the year attains its maximum at Greenwich about the end of July (a considerable time after the summer solstice), and the day of minimum mean temperature occurs in the latter half of January (the intervals between the maximum and minimum, and the minimum and maximum, being as 5·5 to 6·5), so in the tables and curves of lunar temperature for forty-three and fifty years, a longer interval will be found between the day of maximum heat at the moon's first quarter and the day of minimum heat of the last quarter, than between the days of minimum and maximum. Assuming, then, that the earth and the moon absorb heat equally (due allowance being made for the alternate diurnal action of solar and terrestrial radiation in the case of the earth, and the prolonged bi-monthly alternation of solar and lunar radiation in the case of the moon), if we consider the portion of the curve between the days of maximum and minimum as representing the period during which the temperature of the moon is increasing, and the portion of the curve between the days of minimum and maximum as the period during which the temperature of the moon is decreasing, the same causes operating in the case of both planets, there would appear to be actual evidence of similar effects.

Exceptions accounted for.—Whether, however, the moon clears the atmosphere by the agency of her radiant heat, or by thermo-electric currents, or by changing the direction of the winds (a phenomenon not unfrequently, perhaps, itself due to ascending currents caused by lunar radiation), the immediate cause of the phenomenon signalized by the curves would still seem to be the presence or absence of cloud and vapour in the higher regions of the air, and the exceptions to the rule of a period of cloud being on the average a period of heat would be owing to the varying positions of the sun, the moon, and the earth, or to the fact that the formation of

cloud and vapour is due to the sun and the winds, and not in any wise, as it would appear, to the moon, or, lastly, to that system of compensation and alternation which seems to obtain so frequently in atmospheric phenomena, and is so suggestive of mechanical force.

The exceptions to the rule of a higher temperature occurring at the moon's first quarter, and lower temperatures after full moon, in any single year or group of lunations, are not more frequent than occur during the annual march of the seasons, and affect the position of the mean hottest and coldest day in the solar year.

Several curves besides those referred to in the text are appended.

Description of the Curves.—Plate IV. fig. 1. Curve of mean temperature for 618 lunations (1814–65), from the Greenwich observations as corrected by Mr. Glaisher.

Fig. 2. Curve of mean temperature for 99 lunations (1856–64), from the same source.

Fig. 3. Curve of minimum temperature from the Greenwich observations for the same 99 lunations.

Fig. 4. Curve of mean temperature for three years, or 37 lunations (1859–61), at Oxford, from the photographic curves of temperature taken at the Radcliffe Observatory.

Fig. 5. Curve of mean temperature for the same three years as in fig. 4, from the ordinary means of the days at Greenwich, to compare with fig. 4.

Plate V. fig. 1. Curve* of mean temperature for 520 lunations (1814–56) at Greenwich.

Fig. 2. Curve of mean temperature for the 86 lunations (1841–47) during which bihourly observations were taken at Greenwich.

Fig. 3. Curve of mean temperature for 86 lunations (1837–43), from the Ordnance observations at Dublin.

Fig. 4. Curve of mean temperature at Oust Sisolsk (Siberia), for 86 lunations (1837–43), to compare with fig. 3. (Mean of Russian observations at 18^h, 2^h, and 10^h.)

Fig. 5. Curves of minimum temperature for one year (1859) at Greenwich and Utrecht.

Note.—In 1848–56, the maximum occurred on the second day after first quarter, and a second maximum before last quarter. The minimum was found on the third day before first quarter, and the second minimum on the day before full moon.

* This curve appeared in the British Association Reports for 1859.

Plate IV. *Lunar Curves of Mean Temperature.*

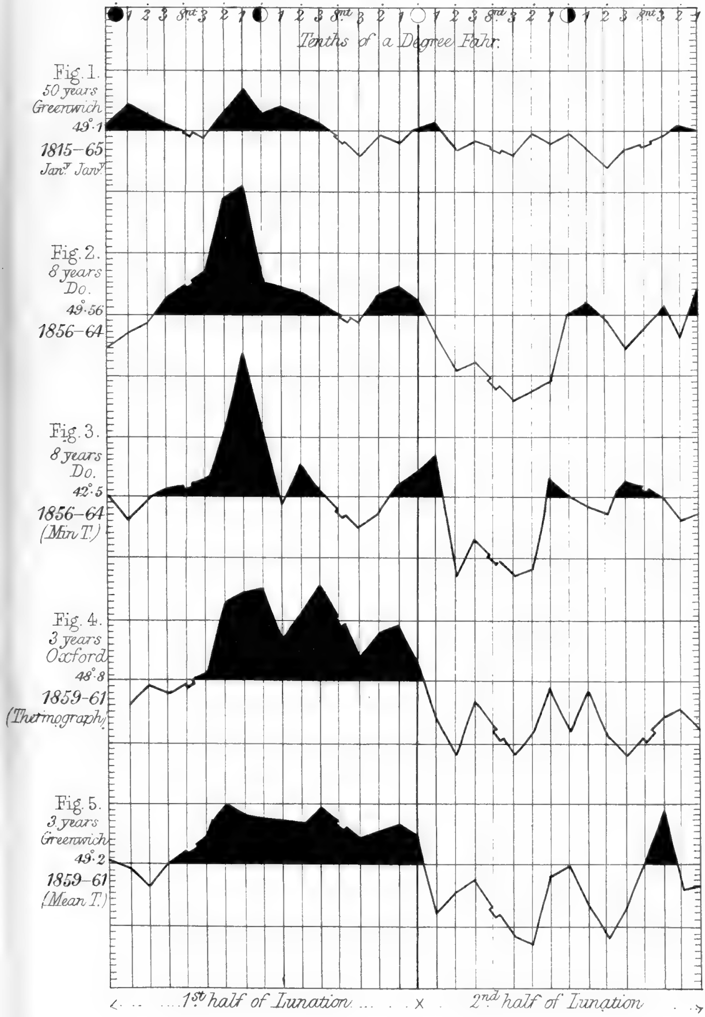
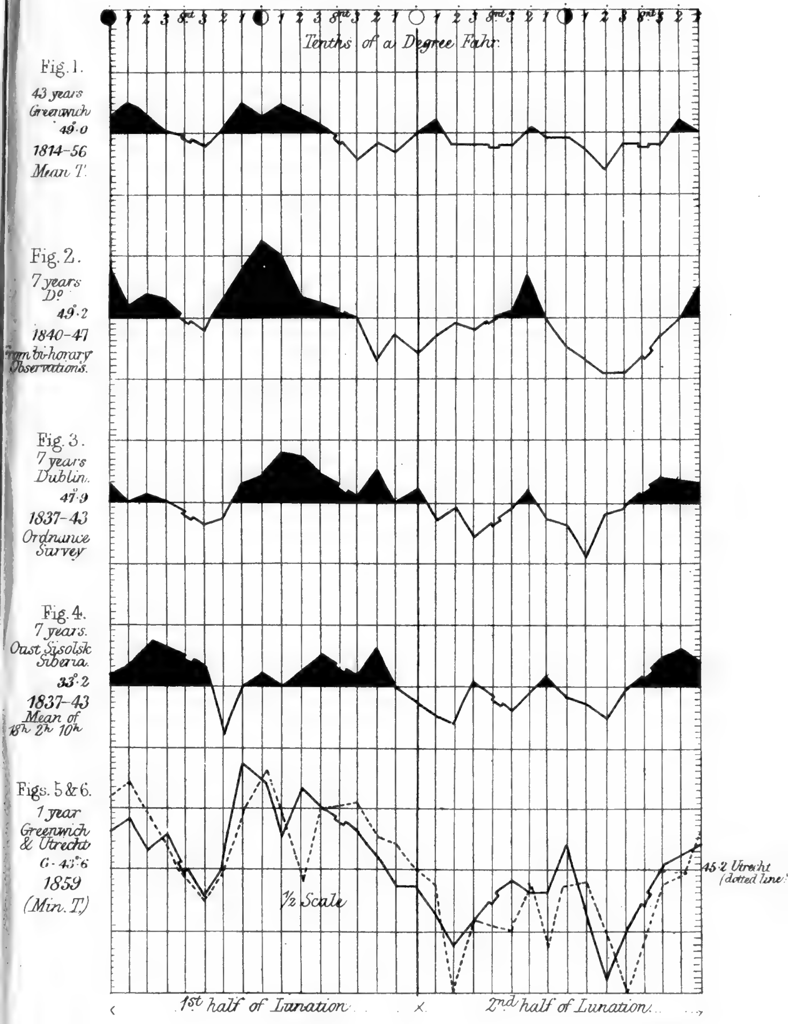


Plate V. *Lunar Curves of Mean Temperature.*



J. Basire lith.

May 11, 1865.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

"On the ultimate Nerve-fibres distributed to Muscle and some other Tissues, with observations upon the Structure and probable Mode of Action of a Nervous Mechanism"*. Being the CROONIAN LECTURE for 1865, delivered by LIONEL S. BEALE, M.B., F.R.S., Fellow of the Royal College of Physicians, Professor of Physiology and of General and Morbid Anatomy in King's College, London; Physician to King's College Hospital.

INTRODUCTION. Of the movements occurring in the tissues of living beings, and of contractility.——THE DISTRIBUTION OF NERVES TO INVOLUNTARY MUSCLE. Distribution of nerves to the muscular fibres of the frog's bladder. Distribution of nerves to the muscular fibres in the walls of arteries, veins, the intestine, ducts of glands, &c. ——THE DISTRIBUTION OF NERVES TO STRIPED MUSCLE. Of the arrangement of the dark-bordered nerve-fibres distributed to voluntary muscle and other tissues. Of the division of dark-bordered nerve-fibres as they approach their distribution. Of the fine fibres running with the dark-bordered fibres. Of the distribution of the pale nucleated nerve-fibres to the elementary muscular fibres. The distribution of nerves to the muscles of articulata. Of the structure of the bodies termed nerve-tufts or -eminences (Nervenhügel) seen in connexion with certain muscular nerves. Of the arrangement of the nerve-fibres in other forms of striped muscle, as the branching muscular fibres of the tongue, the muscular fibres of the heart, and lymphatic hearts of the frog. Of the finest nerve-fibres which influence the muscle.——THE ESSENTIAL STRUCTURE OF A NERVOUS MECHANISM CONSIDERED. Of the supposed terminations of the dark-bordered nerve-fibres, and of the probable existence of nerve-circuits. Of terminal plexuses and networks of fine nerve-fibres in the cornea, pericardium, fibrous tissue of the abdomen, and other parts. Fine nerve-fibres distributed to capillaries in the form of networks and plexuses. Arguments in favour of uninterrupted circuits, deduced from an examination of the trunks of nerves. Of the termination of nerves in papillæ, and in special cutaneous nervous organs, such as the papillæ concerned in touch and taste, and in the Pacinian corpuscles. Evidence, in favour of continuous nervous circuits, derived from the study of the development of nerve-fibres distributed to muscle. Of the relation of the ultimate branches of the nerve-fibres to the tissue and to the germinal matter. Arguments in favour of uninterrupted circuits founded upon the structure and arrangement of ganglion-cells. *General conclusions* deduced from the above facts in favour of the existence in all cases of complete nervous circuits, and of the absence of any interruption in the continuity of nerve-fibres.

It seems to have been the desire of the founder of the lectureship which I have the honour to hold this year, that a lecture or discourse on the nature or property of local motion, accompanied by an experiment, should

* This Lecture will be published shortly in a separate form, with all the drawings. The references made in the text to illustrations apply to the drawings and diagrams exhibited during the delivery of the Lecture.

be delivered annually to the Fellows of the Royal Society. It appears that the subject of muscular motion was selected by many of the earlier Croonian lecturers, and it has been generally considered that the Croonian Lecture should be confined to this department of local motion. Although this view was founded upon a misconception, it would indeed have been difficult to have selected a subject better adapted for frequent and repeated investigation and illustration than muscular motion. Notwithstanding more than a century has elapsed since the first Croonian Lecture was delivered, the nature of muscular motion, and the mechanism taking part in its production, still remain to be discovered. In this as in every other department of natural knowledge, it is to be noticed that the gradual progress made by the unremitting labour of successive observers, so far from exhausting the fields of scientific inquiry, seems but to prepare the way for ever-increasing advance.

By the excellent custom of appointing lecturers to deliver at certain intervals of time lectures upon the same department of natural knowledge, the actual progress achieved from time to time may be distinctly defined and duly registered, and new lines of inquiry suggested for future investigators in the same department. Although I have been led to choose for the subject of my lecture an anatomical question which seems extremely simple and of limited extent, I am compelled to leave many points but imperfectly studied; and notwithstanding I have worked at this question earnestly for several years, my conclusions are in many respects still incomplete.

It is remarkable how the positive determination of a simple question of fact may, as it were, recede as the investigation advances. However minute the detail, more and ever more detail seems required before all doubt can be removed from the mind of the student.

I should not have ventured to ask the attention of the Fellows of the Royal Society to minute and, necessarily, in many respects incomplete anatomical detail, were it not that some broad questions of very general interest are involved in the inquiry I have undertaken; and I think that I shall render what I have to say far less tedious and irksome than it would otherwise be, if I state, in the first instance, the questions proposed for discussion, and the general nature of the inferences I have arrived at.

It seems to me that we can scarcely hope to determine the manner in which the nervous system influences the muscular and other tissues, until we have ascertained how the ultimate branches of nerve-fibres are arranged, and have demonstrated by actual observation, or proved by other means, whether the nerves are disposed so as to form loops or plexuses, or whether they exhibit distinct ends, or terminate in special organs or by becoming continuous with other tissues. And it is impossible to separate from this inquiry the further and wider question, concerning the necessary structure and typical arrangement of a nervous apparatus.

Our view regarding the nature of the force which produces such marvellous phenomena as those known to result from nervous action, will be materially influenced by the conclusion which we are led to accept regarding the fundamental arrangement of the nerve-cells and fibres, central and peripheral.

If it could be shown that a nerve passes from its centre and ends by free terminations, or by becoming continuous with the muscular tissue, we should scarcely adopt the same general conclusion regarding the manner in which the nerve-centre influences the contractile tissue as we should if it were shown that the nerve merely passed amongst the muscular fibres without being necessarily even in actual contact with them, and returned towards, and eventually became connected with, the nerve-centre, without there being any solution of continuity in any part of the circuit. The investigations recorded in this and other memoirs have led me to conclude that nerves invariably form circuits, and that there are in truth no ends at all. I believe that the nerve machinery is a complete circuit, and that the active phenomena are due rather to an alteration in the intensity of the current passing along the nerves, than to its sudden interruption and completion.

In this Lecture I hope to be able to adduce facts which indicate the existence of fibres passing from and towards all central and peripheral nerve-cells.

Before I proceed to the special subject of my Lecture, I must offer a few remarks upon *contractility.* Of late this term has been applied to movements occurring in living organisms which seem to me to be quite distinct in their essential nature from contractility. I cannot hold that the movements occurring in an amœba or white blood-corpuscle are of the same nature as those which occur in muscle, and I cannot, therefore, regard both classes of movements as manifestations of one property, *contractility.*

Of the Movements occurring in Cells and in the Tissues of Living Beings, and on Contractility.

Vital movements.—The peculiar movements occurring in a mass of germinal matter are illustrated by the drawing now exhibited. Protrusions may occur at one or many points of the mass at the same time, and the whole mass may move in one direction like an amœba. Now it will scarcely be maintained by any one that the changes of form occurring in a mass of living matter are due to external agency. As far as can be ascertained by observation, the movement is primary, and depends upon the active forces inherent in the matter itself. This form of motion has never been explained or accounted for; but as it ceases with the death of living matter, it is only reasonable to infer that it is intimately associated with those other phenomena which are peculiar to matter in the living state. It may therefore be termed *vital motion,* to distinguish it from every other kind of movement known. The rotation and other movements affecting the "pro-

toplasm" of certain vegetable cells, and the motion of masses of germinal matter in various tissues of man and animals must be included in this class of vital movements*.

Ciliary action is, I think, a secondary phenomenon, due to changes going on within the cell, but probably very intimately connected with the currents flowing to and from the germinal or living matter, and the altered tension thus caused in the cell. Ciliary motion is not dependent upon nervous action, nor is it due to any disturbance in the surrounding medium. Ciliary motion cannot be regarded as a *vital* movement, although it is probably due to changes which are consequent upon vital phenomena. Cilia consist of "formed material."

* No one will be more ready to receive and acknowledge that these movements and other phenomena characteristic of living matter are due to ordinary force than myself, so soon as the correctness of such a doctrine shall have been proved, or, indeed, any advance towards this end shall have been made; but as a working physiologist desiring to see, and promote to the utmost, real advance in this department of science, I consider it a duty to oppose as strongly as I can the practice pursued by some scientific authorities in the present day, and especially in this country, of reiterating the assertion that all the phenomena of living beings are to be accounted for by the action of ordinary force. Nothing can retard true progress more than exaggerated statements with reference to advance in any special direction. It is almost certain that the manifest anxiety to substitute for quiet proof intense and positive language, merely indicates bias, if not prejudice, in favour of views not supported by facts. I have already stated, not only that the doctrine does not rest upon any sound evidence whatever, but have drawn attention to the phenomena which occur in the simplest form of living matter, which never have been, and which I believe cannot be explained upon any known physical or chemical laws. Instead of these objections being answered, or the challenge to consider the matter in detail being accepted, we are told that the "tendency of modern science is towards this" apparently much-desired "end, and that although living matter cannot yet be prepared by man, the day is not far distant when its artificial production will be rendered possible," and so forth!

The fallacy underlying many of the modern doctrines is obscured, if not entirely concealed, by the very ingenious choice of words. For instance, when it is stated, with what appears to be learned precision, that force is "conditioned" by the "*molecular machinery*" existing in the cell, few probably would be led to inquire what the molecular machinery was, and how the "conditioning" took place. Now it so happens that the changes in question occur without the existence of anything to which the term machinery can be properly applied. Instead of the living cell being like a machine, it is perhaps less like a machine than anything else that we have any knowledge of. The "living machine" is just a very minute mass of soft, colourless, transparent, semifluid matter, endowed with very wonderful properties or powers, in which matter is decomposed and its elements rearranged, while its forces are conditioned in a manner that cannot be effected by man with the aid of the most perfect machinery and elaborate apparatus his ingenuity has devised. Living matter is not a machine, nor does it act upon the principles of a machine, nor is force conditioned in it as it is in a machine, nor have the movements occurring in it been explained by physics, or the changes which take place in its composition by chemistry. The phenomena occurring in living matter are peculiar, differing from any other known phenomena; and therefore, until we can explain them, they may be well distinguished by the term *vital*. Not the slightest step has yet been made towards the production of matter possessing the properties which distinguish living matter from matter in every other known state.

In the immediate vicinity of ciliated cells are sometimes observed cells with open mouths, out of which mucus and various substances, formed or secreted in the interior of the cell, pass. In the formation of these products, nutrient matter from the blood, after passing through the attached extremity of the cell, is probably absorbed by the living matter. At the same time the outermost portion of the latter becomes converted into the peculiar contents of the cell, and thus the formed matter which has been already produced becomes pushed towards the orifice. This is explained by the drawing; and I think that the movements of cilia are brought about by a somewhat similar series of changes, in which the germinal or living matter, usually termed "nucleus," plays the active and most important part.

Movements of granules within cells.—The movement of insoluble particles from one part of a cell to another, as occurs in the radiating pigment-cells of Batrachia, is probably due to alteration in the direction of the flow of fluid in the cells, *from* the cavity of the cell *towards* the tissues, or *from* the surrounding tissue *into* the cell. If the capillaries were fully distended, fluid would permeate their walls and would pass into the cavity of the cells, in which case the insoluble particles would gradually become diffused and would pass into all parts of the cell; while, on the other hand, if the capillaries were reduced in diameter, and the lateral pressure upon their walls reduced, there would be, as is well known, a tendency for the fluid in the surrounding tissue to flow towards the vessels and pass into their interior. In this case the quantity of fluid in the cells would be gradually reduced, and the insoluble particles would become aggregated together and would collect in those situations where there was most space, as in the central part of the cell, around the nucleus. Moreover, in the last case, the flow of fluid, which constantly sets towards the nucleus, would be instrumental in drawing the particles in this same direction, while if the cell contained a considerable proportion of fluid, the currents would pass between the particles without moving them. Evaporation, as it occurs after death, causes concentration of the insoluble particles towards the centre of the cells.

On the other hand, the changes in the pigment-cells of the frog have been considered by Professor Lister to be due to *vital actions*, and he agrees with Wittich and others that they are under the immediate control of the nervous system. Indirectly of course they are, but I do not think that any experiments have proved satisfactorily that the nerves exert any *direct* influence upon the movements of the particles in these cells. It is well known that the nerves govern the calibre of the vessels, and thus influence the amount of fluid in the surrounding tissue, and in this indirect manner they doubtless affect the movements of the particles in the cells. The reader will find a full account of Prof. Lister's experiments, and the arguments deduced from them, in his paper "On the Cutaneous Pigmentary System of the Frog," published in the Philosophical Transactions for 1858.

Muscular movement is illustrated by the figures to which I now refer,

and which are intended to show the alterations in form supposed to take place in the ultimate particles of any contractile tissue—movements occurring in definite directions, which may be represented by lines at right angles to one another. These movements are quite distinct from those varied movements in all directions which affect matter in the germinal or living state. Contractile tissue is formed material, and contractility occurs in tissue which does not exhibit any of those properties or powers which distinguish living matter. It seems to me, therefore, that contractility is not a *vital property*; and I think that the term *contractility* should be restricted to movements which are remarkable for their constant repetition, and for the simplicity of their character. The changes which occur in the particles of a muscle during action might be spoken of as alternate *shortening* and *lengthening*.

Experiment.

The phenomena of contractility can be studied more satisfactorily in the muscles of the common maggot or larva of the blow-fly than in those of any other animal I am acquainted with. The movements, which are very beautiful, continue for ten minutes or a quarter of an hour after the muscles have been removed from the body of the recently killed animal; and I hope to be able to prepare a specimen which can be passed round in one of the portable microscopes and examined by the Fellows. [Preparation sent round.] In the winter I have seen the contractions continue for upwards of half an hour. But the most beautiful and instructive method of examination is under the influence of polarized light, with a plate of selenite. In the microscope upon the table, the arrangement has been made; and when the ground is green, the waves of contraction which pass along each muscular fibre in various directions, are of a bright purple. In other parts of the field the complementary colours are reversed. There are few microscopic objects, that I am acquainted with, so beautiful as this. With the aid of very high powers, the actual change occurring in the contractile tissue as it passes from a state of relaxation to contraction, and from this to relaxation again, may be studied, and for many minutes at a time*.

Molecular movements.—The cause of the so-called molecular movements is probably complex, but quite independent upon any phenomena peculiar to living beings.

The various movements occurring in the ultimate elementary parts or "cells" of living beings may be arranged as follows:—

1. *Primary or vital movements.*—Affecting matter in the living state only and occurring in every direction, as seen in the amœba, white blood-corpuscle, mucus- and pus-corpuscle, young cells of epithelium, and in germinal matter generally.

* The character of muscular movements is fully described in Mr. Bowman's well-known paper (Phil. Trans. 1841). See also Mr. Bowman's article, "Muscular Motion," in Todd's Cyclopædia of Anatomy and Physiology.

2. *Secondary movements*—the consequence of vital movements, or of other phenomena, affecting matter which is not in a living state:—

a. *Ciliary movements.*—Probably due to alterations in the quantity of fluid within the cell, the changes in the proportion of fluid being brought about by the action of the living or germinal matter in the cell.

b. *Movements of solid particles suspended in fluid in cells, caused by currents in the fluid,* as the pigmentary matter in the pigment-cells of the frog.—Due to the motion of the fluid as it passes into or out of the cell through its permeable wall, this movement being dependent upon changes taking place external to the cell.

c. *Molecular movements.*—Which affect all insoluble particles, in a very minute state of division when suspended in a fluid not viscid.

d. *Muscular movements.*—Due to a disturbance (electrical or otherwise) in the neighbourhood of a contractile tissue—that is, a structure so disposed that its constituent particles are susceptible of certain temporary alterations in position, which alterations take place in certain definite directions only.

Distribution of Nerves to Involuntary Muscle.

Distribution of nerves to the muscular fibres and other tissues in the bladder of the frog.

The demonstration of the ultimate arrangement of the most minute nerve-fibres is a matter of such great difficulty that the anatomist is compelled to search with the utmost care for a texture the natural structure of which happens to be favourable for his investigation. There are very few textures which possess so many advantages as the bladder of the frog. It is so thin and transparent, that it may be regarded as a natural dissection and thinning-out of some of the most delicate tissues. The unstriped muscular fibres of this organ are extremely fine, and are slightly separated from one another. Nerve-fibres can often be seen in the intervals between the fibres.

I have therefore selected this for illustrating the ultimate ramification of the nerve-fibres in involuntary muscle; but I believe the statements which I shall make will be found to apply with equal force to every variety of this particular form of muscular tissue.

With regard to the presence of nerve-fibres in involuntary muscle, I may remark that nerve-fibres have been demonstrated in so many different cases, that it is more in accordance with the positive knowledge already gained to infer that they exist in relation with every form of contractile tissue, even in cases in which we may still fail to demonstrate them, than to infer they are absent simply because we have failed to render them distinct*.

* By contractile tissue I mean a tissue in which simple movements like shortening and lengthening alternate with one another, each movement being a mere repetition of the first movement that occurred when the *formation* of the contractile tissue was complete.

The bundles of dark-bordered fibres which may be traced to the posterior part of the frog's bladder divide and subdivide freely, spreading out in the form of a lax network, the fibres constituting which may be followed for some distance, and many may be traced to their ultimate distribution in the thin tissue of the bladder. Over a great part of the frog's bladder, however, no dark-bordered fibres or bundles of moderately coarse fibres can be detected; yet the organ is in every part very freely supplied with nerves.

Bundles of excessively fine fibres, first described by me*, may be traced running parallel with many of the small arteries, and may be seen to divide and subdivide into finer bundles, which at length form a plexiform network. Here and there is seen a plexus of very fine fibres, from which bundles of fine fibres diverge in different directions. That very many of these fine fibres come from the numerous ganglion-cells found in connexion with the nerve-trunks there is no doubt; and it is equally certain that many also result from the division and subdivision of dark-bordered fibres. But whether the large dark-bordered fibres seen in the nerve-trunks pass directly to their distribution in the bladder, or in the first place become connected with ganglion-cells, it is difficult to decide with absolute certainty; I have, however, traced several of the large fibres directly from the trunks to their distribution, but even in these instances I am not prepared to assert that no branches pass to the ganglion-cells. My impression is that sometimes this is the case, but that some of the fibres pass to their distribution without there being any such connexion with ganglion-cells; and I think it probable that, of the fibres resulting from the division of a dark-bordered fibre derived from the cord, some may become connected with ganglion-cells, while others may pass to their distribution in the bladder without being connected with these cells.

In the very thin membrane of which the walls of the frog's bladder are composed we may follow out the distribution of nerves—*a* to the muscular tissue, *b* to the surface of the mucous membrane, *c* to the vessels, and *d* to the connective tissue.

In this drawing the general arrangement of the nerve network is represented, from which fibres pass to supply all the tissues of the bladder.

Upon the external surface of the lung of the frog muscular fibre-cells exist in small number, and to these a network of delicate nerve-fibres is distributed. These muscular and nerve-fibres are, however, much more highly developed upon the newt's lung than upon that of the frog. But in this Lecture I restrict myself to the consideration of the distribution of nerves to the muscular fibre-cells, which is described in very few words and will be at once understood by reference to the diagram to which I now direct attention.

The muscular fibre-cells of the bladder itself and of the small arteries

* "On very fine Nerve-fibres in Fibrous Tissues, and on Trunks composed of very fine Fibres alone" (Archives of Medicine, vol. iv.).

are crossed sometimes in two or three places by very fine nerve-fibres; and not unfrequently the nerve-fibre runs parallel with the muscular fibre-cell for some distance.

These nerve-fibres are extremely fine, and require very high powers for their demonstration. They are certainly not connected in any way either with the nucleus or with the contractile tissue of the muscular fibre. They cross the fibre either obliquely or at right angles; and oftentimes a nerve-fibre runs for some distance parallel with the muscular fibre. The influence, therefore, exerted by the nerve-fibre cannot depend upon any continuity of texture between it and the contractile tissue, but is doubtless due to the passage of a current through the nerve, which determines a temporary alteration in the relations to one another of the particles of which the contractile tissue consists.

Although I speak of the ultimate nerve-fibres as being arranged so as to form a network, it must not be supposed that this network is arranged on the principle of a capillary network. Every fibre of this network is compound; so that perhaps the term "plexus" more truly describes the arrangement. "Plexiform network," I think, expresses the character of the arrangement still more exactly*.

Some have said that my view accords with the old idea of looplike terminations of nerves; and this is in the main true, but the course of one single fibre forming the loop is far more extensive than was supposed by the older observers, and the "looped fibres" divide and subdivide into finer fibres. This diagram is intended to represent a plan of the arrangement which is shown to exist in many tissues according to my observation.

Although it be admitted that networks are formed, it might be said that from them fine fibres may branch off here and there, and *terminate* in ends within the space or area. The results of actual observation and a careful consideration of the various facts bearing upon the question are, however, strongly opposed to such a doctrine.

Distribution of nerves to the muscular fibres in the walls of arteries, veins, the intestine, ducts of glands, &c.

So far as I can ascertain, all involuntary muscular fibre is freely supplied with nerve-fibres, and in all cases the nerves are arranged so as to form networks. In many instances ganglia are seen in connexion with the nerves ramifying amongst the muscular fibre-cells encircling the vessels. I have seen such upon the vessels of all the viscera and those of the palate

* "In using the term network, I do not mean to imply that fine nerve-fibres unite with each other after the manner of capillaries, but merely that the *bundles* of fibres are arranged like networks. The fibres composing the bundles do not anastomose. In lace the appearance of a network of fibres is produced; but every apparent thread is composed of several, each of which pursues a complicated course, and forms but a very small portion of the boundary of any one single space. In Plate XLI. fig. 5, a nervous network exists; but each cord is compound, and composed of numerous fibres which never anastomose."—*Note appended to a paper in the Phil. Trans.* 1862.

of the frog: they are to be detected upon the iliac arteries in considerable number. The results of Mr. Lister's experiments render it probable that ganglia exist in connexion with the arteries of the limbs (Phil. Trans. pt. 2 for 1858, p. 620).

In this figure a small ganglion in course of development upon one of the iliac arteries of the frog is represented; and several fine branches of nerve-fibres can be followed amongst the muscular fibre-cells. I have seen very fine nerve-fibres beneath the circular muscular fibre-cells, apparently lying just external to the lining membrane of the artery, and composed of longitudinal fibres with elongated nuclei—an observation which confirms a statement of Luschka's. I have not succeeded in satisfying myself that nerve-fibres are ever distributed to the lining membrane of an artery, although, from the appearances I have observed, I cannot assert that this is not the case. In the auricle of the heart and at the commencement of the venæ cavæ, very fine nerve-fibres are certainly distributed very near indeed to the internal surface, being separated from the blood only by a very thin layer of transparent tissue (connective tissue).

The distribution of nerve-fibres to the coats of a small artery about the $\frac{1}{800}$th of an inch in diameter is represented in this drawing. In all cases (and I have examined vessels in almost all the tissues of the frog), not only are nerve-fibres distributed in considerable number upon the external surface of the artery, ramifying in the connective tissue, but I have also followed the fibres amongst the circular fibres of the arterial coat. The nerves can be as readily followed in the external coat as in the fibrous tissues generally; and the appearance of the finest nucleated nerve-fibres, already alluded to, enables one to distinguish them most positively from the fibres of the connective tissue in which they ramify.

These nerves invariably form networks with wide meshes. I have demonstrated such an arrangement over and over again. A similar disposition may be seen in the auricle of the frog, in the coats of the venæ cavæ near their origin from the auricle, among the striped muscular fibres of the lymphatic hearts of the posterior extremities of the frog, and in other situations. Kölliker confesses that he has not succeeded in observing distinct *terminations* to the nerves distributed to the vessels of muscles. He states that some arteries are completely destitute of nerves, and, apparently without having given much attention to the subject, says "hence it is evident that the walls of the arteries are not in such essential need of nerves as is usually supposed." It is easy to demonstrate nerves in considerable number on all the arteries of the frog, and in the case of certain vessels of man and the higher animals in which we have failed to demonstrate nerves, it is more reasonable to assume that they are there, although they have not been seen, than to infer their absence simply because we have failed to render them distinct. In the case of the umbilical arteries of the fœtus and their subdivisions in the placenta, it is quite certain there are no true dark-bordered nerve-fibres, but we now know that the active part of a nerve

may consist of an exceedingly delicate, pale, and scarcely visible fibre, connected with a nucleus. Such delicate fibres and nuclei are to be demonstrated amongst the muscular fibres of these arteries, but in consequence of not having been able to trace them continuously for any great distance, I cannot assert that these are true nerves; but no one has yet proved they are not nerves, or has demonstrated their real nature.

The nerves which supply the small arterial branches in the voluntary muscles of the frog, come from the very same fibres which are distributed to the muscles. I have seen a dark-bordered fibre divide into two branches, one of which ramified upon an adjacent vessel, while the other was distributed to the elementary fibres of the muscle. In my paper "On the Structure of the Papillæ of the Frog's Tongue" these statements have been confirmed; and in the figure to which I now point, nerves distributed to arteries and to elementary muscular fibres of striped muscle are seen to be derived from the same trunk of dark-bordered nerve-fibres.

Distribution of Nerves to Striped Muscle.

Of the arrangement of dark-bordered fibres distributed to voluntary muscle and other tissues.

The plexiform arrangement of nerve-trunks and nerve-fibres is one which is very general, and was known even to the older anatomists. It can be demonstrated in many cases even by rough dissection. It exists not only in the case of nerves distributed to muscle, but, as far as is known, to every other tissue which receives a supply of nerves. Many of these networks are very beautiful; and the arrangement is illustrated by these figures, which represent the bundles of dark-bordered nerve-fibres distributed respectively to the diaphragm of the white mouse, the mylohyoid of the green tree-frog, and the eyelid of the same animal. The fibres constituting the bundles never run perfectly parallel with one another, nor can a separate fibre usually be followed for any great distance. This arises from the fact that the fibres frequently cross one another, and many seem to pursue a spiral course. The spiral arrangement of nerve-fibres has been already described in former communications. At an early period of development one fibre may be seen coiled spirally round the other, as is well shown in this drawing*. The rule seems to be universal that fibres on one side of a trunk cross over and pursue their course on the opposite side. Those on the lower part of a trunk soon pass to the upper part, and *vice versâ*. Instead of a nerve passing to its distribution by the shortest route, it invariably seems to pursue a very circuitous course. The course of the nerve-fibres in the optic commissure is not peculiar to this part of the nervous system, but a similar arrangement is to be met with in all nerves. When two trunks meet, as represented in this figure, fibres are found to pursue the several courses represented by the lines.

* See also my paper "On the Structure of the so-called Apolar, Unipolar, and Bipolar Nerve-cells," Phil. Trans. 1863.

Division of the dark-bordered nerve-fibres as they approach their distribution.

It is to be specially noted that the dark-bordered fibres very frequently divide, and, in consequence of the fibres being exceedingly thin at the points of division, which occur, for the most part, just where a bundle of fibres divides into two branches, they are seen only in very carefully prepared specimens. Although Wagner long ago showed that dark-bordered fibres underwent subdivision, the numerous subdivisions which I have demonstrated in all dark-bordered fibres near their peripheral distribution and also as they pass towards the nerve-centre, have not been generally observed. The number of fibres into which a single dark-bordered fibre divides is very great in a comparatively short course. The resulting subdivisions pursue very different directions, and can often be followed for a considerable distance as they run with other dark-bordered fibres. From this it follows that many different parts of a muscle at a distance from one another may be supplied with nerves which result from the subdivision of a single dark-bordered fibre.

The fibres resulting from the subdivision of the dark-bordered fibres are of less diameter than the parent trunks; but the area of the section of two fibres would invariably be much greater than that of the parent trunk. For the most part the fibres divide dichotomously; but sometimes a fibre is seen to divide into as many as three or four divisions, and in muscle five, six, or even more dark-bordered fibres have been seen to result from the division of one. The finer dark-bordered fibres often run in the same bundle with coarse dark-bordered fibres, the former being in fact much nearer to their ultimate destination than the latter. The dark-bordered fibres distributed to the muscles of the frog often divide into two very fine fibres, as represented in several of these figures. These fibres may be traced onwards for some distance. They do not exhibit the dark-bordered character. They appear pale and granular, and connected with them at varying intervals are nuclei. These pale nucleated fibres in the frog are often less than the $\frac{1}{50000}$ of an inch in diameter. They are nevertheless compound, and consist of bundles of still finer fibres. These in fact, although much narrower, correspond to the pale, granular, but nucleated intermuscular nerves first described by me in the muscles of the mouse (Phil. Trans. 1860). The very fine compound fibres still continue to divide and subdivide, and assist to form plexuses and networks in precisely the same manner as the dark-bordered fibres, of which they are the continuation. It is quite certain that these pale fibres are true nerve-fibres, for they are directly continuous with the dark-bordered fibres. Instead of breaking up into one or more bundles of fine fibres, a dark-bordered fibre not unfrequently divides into a finer dark-bordered, and a bundle of fine fibres, as represented in this drawing from the frog's mesentery.

Of the fine fibres running with the dark-bordered fibres.

We find in the same nerve-trunk fine and coarse dark-bordered fibres, and we often observe exceedingly fine pale fibres running with dark-bordered fibres, the essential difference between these two classes of fibres in the same trunk being that the former fibre is nearer to its ultimate distribution than the latter; but in some instances it is probable that the fine fibre is a branch of the sympathetic. The fine fibre runs in the same transparent matrix (sheath) with the dark-bordered fibre. In fact the idea of tubular membrane or sheath being an essential and separate anatomical constituent of every individual dark-bordered fibre must be given up. For, as I showed in 1860, several dark-bordered fibres and fine fibres might run together in the same sheath or matrix. The opinion that the fine fibres which I hold to be nerve-fibres running in the same sheath with the dark-bordered fibres, are not nerve-fibres at all, but modified connective tissue, is, however, still entertained by many observers. As I have before stated, their continuity with true dark-bordered fibres may often be seen, and the same fibre may in some instances be followed to its ultimate distribution.

The different and incompatible views existing between continental observers and myself are in some measure due to this sheath question. The so-called sheath is not a "tube" or "membrane," or "tubular membrane," which contains the other constituents of the nerve-fibre; nor is it a sheath which invests them, but it is simply a transparent matrix, in which nerve-fibres, coarse and fine, are imbedded. The so-called sheath is not formed as a special structure to invest the nerve-fibres, but it results from changes occurring in the nerve-fibres themselves. This "sheath" or "tubular membrane" of the so-called dark-bordered fibre precisely corresponds to the transparent connective tissue, in which the fine nerve-fibres are imbedded. It is a form of connective tissue, and in many situations where nerves existed at an earlier period, nothing but this so-called sheath remains. All the soluble fatty matters have disappeared, and this material, which is not readily absorbed, is left behind. Vessels may waste, and ducts and glands may waste, and leave behind them the same sort of transparent connective tissue. Moreover, as I have before stated, it is altogether a fallacy to suppose that near the peripheral distribution, every single branch of nerve-fibre is surrounded by its own separate sheath. The drawings of the so-called axis-cylinder near the terminal distribution of the nerves also seem to me to be diagrammatic, founded rather upon a theoretical idea of the constitution of the nerve-fibre than upon the results of actual observation.

Many of the pale fibres accompanying the dark-bordered fibres are doubtless sympathetic fibres, for it has been shown that there are fine fibres springing from ganglion-cells which retain the same character from their origin to their distribution (see p. 236). Not only has the nervous nature of the fine fibres above described been proved by tracing them from their connexion with ganglion-cells, but a dark-bordered fibre has often been observed to be drawn out so as to form a line as fine as these fine

fibres. Indeed the observer often fails to trace an individual dark-bordered fibre for any great distance in consequence of its becoming exceedingly fine at the point where it crosses, or is crossed by other dark-bordered fibres. Not only so, but where a bundle of comparatively wide dark-bordered fibres passes through a small aperture, as for example in a bone, the fibres appear, as it were, drawn out to exceedingly thin threads, as represented in this figure.

And it may be fairly argued that since a wide dark-bordered fibre may be reduced in certain parts of its course to a fine cord not more than the $\frac{1}{50000}$th of an inch in diameter, without its integrity or conducting-power being interfered with, there is nothing unreasonable in concluding that fine fibres of the same diameter are efficient conductors of the nerve-current, although their length may be considerable. And I have shown that in many of the tissues of the frog (bladder, connective tissue, auricle, &c.), the finest branches of the nerves at their distribution are invariably less than the $\frac{1}{60000}$th of an inch in diameter. Is it then probable that the nerves distributed to the elementary fibres of the voluntary muscles of the limbs should form the single exception to this very general arrangement, and that the peripheral nerves of muscle should exhibit the dark-bordered character up to, or to within, a very short distance of their ultimate distribution or termination, as is maintained by many German anatomists.

Of the distribution of the pale nucleated nerve-fibres to the elementary muscular fibres.

Few anatomical questions have received of late years a larger share of attention than the ultimate arrangement of nerve-fibres in voluntary muscle. It is a matter of regret to me that although I have studied the question in many ways during the last five years, my conclusions do not accord with those of any other observer. And I must admit that although the German writers differ from one another on not unimportant points, they, nevertheless, agree in this, that the nerves form ends, pass into end-organs, or exhibit terminal extremities of some kind; while on the other hand my observations have led me to conclude not only that nerves never terminate in ends in votary muscle, but that there are no terminal extremities or ends in any nervous organ whatever.

With regard to the ultimate arrangement of nerves in muscle, the conclusions of Kölliker accord more nearly with my own than those of any other observer. (Compare Kölliker's statements in his Croonian Lecture delivered in 1862, with the results stated in my paper, published in the Phil. Trans. for 1860.) Kölliker agrees with me in the opinion that the nerves lie upon the external surface of the sarcolemma; but what he regards as ends or natural terminations, I believe to be mere breaks or interruptions in fibres which in their natural state were prolonged continuously.

And there is this further broad difference between foreign observers and myself, that while they consider that each elementary muscular fibre is very

sparingly supplied with nerves—a very long fibre receiving nervous supply at one single point only—I have been led to conclude that every muscular fibre is crossed by very delicate nerve-fibres, frequently, and at short intervals, the intervals varying much in different cases, but, I believe, never being of greater extent than the intervals between the capillary vessels.

My friend Kühne, of Berlin, has probably published more papers upon this vexed question than any other observer. He maintains that the nerve always passes through the sarcolemma and comes into direct contact with the contractile tissue*, or ends in protoplasmic matter which is in continuity with the muscle. He has, however, from time to time been led to modify his view very materially, as these figures, copied from his various memoirs published between the years 1862 and 1864, will testify. In his memoir published in 1862, he described minutely the structure of some very peculiar organs, which he stated had been demonstrated by him in connexion with the pale terminal intramuscular branches of the nerve-fibres. In more recent memoirs he seems to have abandoned the idea of the existence of those very peculiar bodies which he termed "Nerven-Endknospen," and with reason, since no other observer professes to have seen objects at all resembling those figured by Kühne. I should, however, state that the observations of Kühne have in the main been supported by Engelmann and some other observers.

In this Lecture I am unable to give even a brief account of the different views now entertained by the numerous observers who have studied this question; but in these drawings some of the most important are represented. A record of the opinions entertained by various writers will be found in Kühne's memoir, published in Virchow's 'Archiv,' vol. xxx. 1864; and I append the titles of some of the most important communications which have appeared since the publication of my first memoir:—

Kühne. Note sur un nouvel organe du système nerveux.—Comptes Rendus, Feb. 1861.

Kühne. Ueber die peripherischen Endorgane der motorischen Nerven. —Leipzig, 1862.

Theodor Margó. Ueber die Endigung der Nerven in der quergestreiften Muskelsubstanz.—Pest, 1862.

Kölliker. Untersuchungen über die letzten Endigungen der Nerven.—1862.

Rouget. Note sur la terminaison des nerfs moteurs dans les muscles chez les reptiles, les oiseaux et les mammifères.—Comptes Rendus, Sept. 20th, 1862; also Brown Séquard's Journal, 1862.

Naunyn. Ueber die angeblichen peripherischen Endorgane der motorischen Nervenfasern.—In Reichert und Du Bois Reymond's Archiv, 1862, p. 481.

* This view was first advanced by Kühne in 1859 ("Untersuchungen über Bewegungen und Veränderungen der contractilen Substanzen," Reichert und Du Bois Reymond's Archiv, 1860).

L. Beale. Further observations on the Distribution of Nerves to the Elementary Fibres of Striped Muscle.—Phil. Trans., June 1862.

Krause. Ueber die Endigung der Muskelnerven.—Henle und Pfeufer's Zeitschrift, 1863, p. 136.

Th. W. Engelmann. Ueber die Endigungen der motorischen Nerven in den quergestreiften Muskeln der Wirbelthiere.—Centralblatt f. d. Medic. Wissensch. 1863.

L. Beale. On the Anatomy of Nerve-fibres and Cells, and on the ultimate Distribution of Nerve-fibres.—Quarterly Journ. of Mic. Science, April 1863.

L. Beale. Further observations in favour of the view that Nerve-fibres never end in Voluntary Muscles.—Proceedings of the Royal Society, June 5, 1863.

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L. Beale. An Anatomical Controversy. The distribution of Nerves to Voluntary Muscle. Do nerves terminate in free ends, or do they invariably form circuits and never end?—Archives of Medicine, vol. iv. 1865. Published separately: Churchill, London; Denicke, Leipzic.

L. Beale. On the Structure and Formation of the Sarcolemma of Striped Muscle, and of the exact relation of the nerves, vessels, and air-tubes (in the case of insects) to the contractile tissue of Muscle.—Trans. Mic. Society, 1864.

Rouget. Sur la terminaison des nerfs moteurs chez les Crustacés et les Insectes.—Comptes Rendus, Nov. 21, 1864.

As the observations of Kölliker, Kühne, and other observers in Germany, who followed me, were made upon the breast-muscle of the frog, while my first inquiries were instituted upon the muscles of the white mouse, I subjected this particular muscle of the frog to the same process of investigation which I had previously adopted in my researches in 1858–59, which were published in 1860. The results of these investigations will be understood by reference to these drawings, most of which were printed in my paper published in the Philosophical Transactions for 1862; and as explanations are appended to these figures, it is unnecessary to describe them more minutely here.

Although the results of this further inquiry (published in 1862) were favourable to the view I had advanced, they were deficient in this most

important point, viz. that the supposed network (as seen in this scheme) had not been conclusively demonstrated over the frog's muscular fibres generally. Near the point where the dark-bordered fibre divided to form pale fibres, a network was demonstated as is here shown, but it could not in many instances be traced for any great distance from this point. The following points, however, seem to me to have been established in this memoir:—

1. That the nerve-fibres, as I had already stated and as was confirmed by Kölliker, were outside the sarcolemma.

2. That the fibres might be followed for a greater distance from the dark-bordered fibre than they had been traced before, if the specimens were prepared according to the new method of investigation which I described.

3. The fine pale fibres were proved to be composed of several finer fibres, which resulted from the division of the dark-bordered fibre, and the pale fibres in the sheath of the nerve, which were also demonstrated for the first time.

4. Contrary to the statements of Continental observers, it was proved that the elementary muscular fibres of the frog were crossed at *numerous* points by nerve-fibres, and that the nervous supply to each elementary muscular fibre was much more free and uniform than was supposed. This fact may be demonstrated more especially in the thin muscles of the eye and in the mylohyoid of the frog.

Not satisfied with these results, I examined numerous other muscles of the frog and other animals, in the hope of being able to demonstrate the finest nerve-fibres in every part of their course over the sarcolemma, but was not able to obtain any muscle in the common frog sufficiently thin to trace the finest branches over a very considerable extent of surface. In the *mylohyoid* of the Hyla, however, I found a muscle eminently adapted for this investigation; and on June 5th, 1863, I presented a paper (published in the 'Proceedings') to the Royal Society upon the arrangement of the nerves in this beautiful muscle. I have prepared many specimens in which the nerve can be followed from one undoubted nerve-trunk to another, dividing and subdividing in its course, so as to form with other nerves a lax network of compound nucleated fibres, which compound fibres are often less than the $\frac{1}{60,000}$ of an inch in diameter.

The arrangement will be understood by reference to this drawing, which explains itself. I believe that no other explanation of the appearances observed in these specimens, than the one I have adopted, can be offered. In some of the muscles at the root of the tongue, the same arrangement has been demonstrated most distinctly.

More recently I have again studied the elementary muscular fibres from the breast-muscle of the frog, and have succeeded, in many cases, in tracing the fine granular nucleated fibres for a considerable distance. Near the margin of the muscle, I have recently succeeded in following a very fine fibre, resulting from the subdivision of a dark-bordered fibre, into fibres prolonged from what appear to be connective-tissue-corpuscles. The nuclei

of the network of fine nerve-fibres have often been mistaken for the connective-tissue-corpuscles beneath and, in some cases, amongst which they lie; and as old nerve-fibres, as well as other structures, degenerate and leave behind them what is called "connective tissue," a mistake is easily made unless the preparation be very clear*. In this drawing some very fine nerve-fibres, distributed to a portion of muscle at some distance from a dark-bordered fibre, are represented.

The distribution of nerves to the muscles of Articulata.

The highly elaborate and rapid movements of insects would lead to the inference that in them the distribution of nerves to the muscles must be very free. The textures are, however, so very elaborate, and their structure so minute, that the difficulty of demonstration must needs be greatly increased. Kühne's memoir, published in the year 1860, related to the distribution of nerves to the muscles of *Hydrophilus piceus*. He represented the nerve as perforating the sarcolemma, and being distributed almost in a brush-like manner to the contractile tissue. Subsequently he thought the nerve was connected with the line of muscular nuclei; but it was obvious that these were muscular, not nervous nuclei at all, and this view was abandoned. Some other observers have fallen into the same error. Although I have examined the muscles of many insects, and especially those of the *Hydrophilus*, I have been unable to confirm the observations made by some Continental observers.

For illustrating the distribution of nerves to the muscles of insects, I will select the common maggot, the larva of the blowfly. This insect can be obtained in all countries at almost all seasons of the year.

By reference to these drawings it will be seen that my conclusions accord in the most important particulars with those arrived at in my earlier investigations. The drawing-out of the sarcolemma into a sort of eminence at the point where the nerve commences to ramify over it, is well seen in these two figures. This has been mistaken for a special organ by Kühne (Nervenhügel); and it has been inferred that the nerve perforated the sarcolemma at this point.

In his paper in the 'Comptes Rendus' for November 21, 1864, M. Rouget in part confirms my statements regarding the structure of Kühne's 'Doyère'schen Nevenhügel,' and states that, at the Nervenhügel, the nerve-fibre divides into two fine fibres, which may be traced for some distance, and then terminate. "Leur extrémité terminale est légèrement effilée; elle ne présente ni plaques, ni noyaux, ni substance finement granuleuse."

The structure of these so-called Nervenhügel in insect-muscles was described and figured by me in a paper, accompanied by several drawings, read to the Microscopical Society on June 1, 1864, and published in the 'Transactions' on October 1, 1864. Although M. Rouget agrees with me

* This part of the question is considered in my paper published in the Philosophical Transactions for 1864, page 565.

as respects the nature of the Nervenhügel, we are at variance upon the further course and mode of termination of the nerve-fibre, M. Rouget maintaining that it penetrates beneath the sarcolemma and there terminates in a very fine fibre, in contact with a very limited portion of the contractile tissue, while I have been able to trace the nerve for a long distance beyond the point at which he makes it end, and I have seen it dividing into very fine fibres, which form an extended network upon the sarcolemma, as represented in this drawing, to which I beg to direct special attention. M. Rouget's researches lead him to conclude that the arrangement of the nerves in the muscles of Articulata is totally distinct from that met with in Vertebrata. "Il résult de ces faits qu'il n'y a pas d'identité entre les divers modes de terminaison des fibres nerveuses motrices chez les vertébrés et les articulés." On the other hand, my observations lead me to the conclusion that the arrangement is in its essential points the same in all classes of animals. In no case are there nerve-ends, but always plexuses or networks, which are never in structural continuity with the contractile tissue of the muscle.

I have particularly studied the arrangement and distribution of the nerves in the leech. The same facts noticed in p. 258 on the *branching* of nerve-fibres, are observed in the nerves of this animal; and I have been able to obtain many specimens of nerves which could hardly be distinguished from some of the finest dark-bordered fibres of the higher animals. Some of the muscular fibres of this animal are very thin, and are separated from one another by considerable intervals, in which the ramification of exceedingly delicate nerve-fibres can be readily detected, and the nerve-fibres can be followed to their connexion with ganglion-cells. I have made many specimens of the muscles of the leech, and taken several drawings to illustrate these points, but I regret that I am unable to have these copied for this memoir.

Of the structure of the bodies termed nerve-tufts, nerve-eminences, and Nervenhügel, seen in connexion with certain muscular nerves.

I propose now to consider the structure of the peculiar bodies in connexion with the nerves distributed to the muscles of certain animals, described by Kühne, Rouget, Krause, and others. These differ from the bodies first studied by Kölliker in the breast-muscle of the frog, which are referred to in p. 261. I have never been able to demonstrate such bodies as I am about to describe in the muscles of animals generally, although they are exceedingly distinct in the muscles of lizards, as shown by Rouget. I have demonstrated many in the cutaneous muscles of the neck, and in the muscles of the tongue of the chameleon, and shall carefully consider the structure of these.

In the first place, I would remark that these bodies are *external to* the sarcolemma, as may be proved by examination of the specimens. The course of the nerves *to* and *from* these bodies almost renders it impossible

that they could be beneath the sarcolemma, while in many cases the outline of the sarcolemma can be followed underneath them. Secondly, it appears probable that they are a reduplication and expansion of continuous fibres, rather than terminal organs formed upon the extremities of the nerve-fibres; nor would it seem that these organs are essential to the action of nerves upon muscle, since they are only to be demonstrated in the muscles of certain animals. Moreover, as many different forms of these nerve-organs are to be seen in a small piece of muscle, exhibiting different degrees of complexity, we may perhaps by studying them attentively be able to draw a true inference as to their real structure and the mode of their formation.

Kühne's idea of the structure of these bodies is represented in this diagram, which has been copied from his last paper. The interpretation of the appearances here given is totally different from that which I have been led to offer. In my specimens the nerve-fibres entering into the formation of these tufts are seen to divide and subdivide into several branches, which are folded, as it were, upon one another. The fibre in many instances does not consist of the axis-cylinder only, but the white substance may also be detected in connexion with some of the fibres. The nuclei seem to be connected with the finer branches of the nerve-fibres. In fact the organ seems to consist partly of broad fibres, partly of fine fibres formed by the branching, spreading out, and coiling of the fibres resulting from the subdivision of the original nerve-fibres which enter into the formation of the tuft. Moreover I have succeeded in demonstrating that, from various points of the oval coil, branches pass off and run on the surface of the sarcolemma, probably passing on to other nerve-bundles. These fine fibres, which are represented in my drawings, have not been delineated, as far as I am aware, by any previous observer who has examined these bodies. In connexion with every nerve-tuft there seem to be *entering* and *emerging* fibres; and in the majority of instances, fine fibres may be traced from the tuft in several different directions.

When the nerve-tuft is formed, as it were, upon the trunk of the fibre, the entering fibres are more numerous and larger than the emerging fibres. This is probably to be explained by the circumstance that some fibres pass away from each tuft upon the surface of the muscle, and thus establish communications with nerve-fibres which approach the elementary muscular fibre at other points. This drawing explains how, as the muscular fibre grows, the bundles marked *a* and *b* become separated further and further from one another, and the fine communicating fibres connecting them necessarily become so very much drawn out that they are too delicate to be seen upon the surface of the sarcolemma.

And now it must be obvious that these bodies precisely correspond to the bendings and division of the fine dark-bordered fibres at the point where they come into contact with the surface of the sarcolemma, in the breast- and other muscles of the frog. At this point there is always a twisting of fibres with free branching, and the formation of a number of exceedingly

delicate nerve-fibres, the nuclei or masses of germinal matter being very close together, so that a considerable number are to be observed within a comparatively small space. Here a complex network of fibres, the meshes of which are very small, is found. But this plexus or network is not terminal, nor does it result from the branching of a single fibre, as has been represented. *Many fibres* enter into its formation ; and from various parts of it long fine fibres pass off to be distributed upon the surface of the sarcolemma. This is explained in these figures from the frog, from the white mouse, and in this one from the maggot.

It seems most probable that at the situation of these coils the contraction of the muscular fibre would commence, and that, from the nerve-current traversing several fibres collected over a comparatively small portion of muscle, the contraction at these points would be most intense, while it is probable that the contractions commencing at these points would extend, as it were, from them along the fibre in opposite directions.

I consider these nerve-tufts therefore simply as collections of nerve-fibres, differing only from the ordinary arrangement before described, somewhat in the same manner as the compressed nerve-network in a highly sensitive papilla differs from the lax expanded nerve-network in the almost insensitive connective tissue.

Of the arrangement of the nerve-fibres in other forms of striped muscle, as the branching fibres of the tongue, the muscular fibres of the heart, and lymphatic hearts of the frog.

To certain forms of striped muscle in which no distinct membranous tube of sarcolemma can be demonstrated, nerves are freely distributed ; but all attempts to demonstrate end-organs or terminal extremities in such textures have hitherto failed. In the heart the existence of delicate nerve-fibres arranged to form networks is distinct ; and perhaps the most favourable locality for demonstrating these fibres is the auricle of the frog's heart. Bundles of exceedingly fine nerve-fibres, much resembling those in the bladder, can be seen running in different directions and branching amongst the delicate networks of exceedingly fine muscular fibres. Very fine fibres may be observed in thin specimens with the aid of high powers, crossing the fine muscular fibres at different angles, then dipping down in the intervals between them, and being soon lost in consequence of their ramification in the deeper layers.

In this drawing the relation of the nerve-fibre to the finest part of some of the branching muscles of the tongue is represented ; and I have observed an arrangement precisely similar in the case of the muscular walls of the lymphatic hearts of the same animal. The very thin and narrow muscular fibres of the heart and tongue would appear to offer very many advantages for the demonstration of ends and end-organs, supposing them to exist ; but the most careful observation under the most favourable circumstances and with the aid of the highest powers, reveals only delicate nu-

cleated nerve-fibres, forming lax networks, branches of which may often be followed for a very long distance, and then traced into neighbouring nerve-trunks.

Of the finest nerve-fibres which influence the muscle.

It is probable that the active part of the nerve-fibre, as regards the elementary muscular fibre, commences only at the point where the dark-bordered character of the nerve-fibre ceases, and therefore that the most important and most active portion of the peripheral nerve-fibres distributed to muscle, has escaped the observation of many observers. The fibres are extremely delicate, and, like other very fine nerve-fibres, can only be rendered visible by special methods of preparation. Probably every fibre, however fine, is compound, being composed of several finer fibres. Nuclei are invariably found in relation with these fibres, and they vary in number in different cases. The structure and general appearance of the finest nerve-fibres will be understood by reference to the figures.

From the foregoing observations I conclude that the nerve-fibres which are to be regarded as the fibres of distribution are far more delicate and much finer than has been hitherto supposed. The remarks which I make on this head with reference to the ultimate nerve-fibres distributed to voluntary muscle, will apply to the ultimate nerve-fibres distributed to other organs.

In mammalia the ultimate fibres appear as narrow, long, slightly granular, and scarcely visible bands with oval masses of germinal matter, situated at short but varying intervals, as described in my paper published in the Phil. Trans. for 1860. In many reptiles (frog, newt, lizard, snake, chameleon), however, these ultimate nerve-fibres are narrower but much firmer than in mammalia; and they are more readily demonstrated, as they do not give way under the influence of considerable pressure and stretching. Although fine nerve-fibres have been described in certain situations before I drew attention to these fine pale nucleated fibres in muscle, it was not generally supposed that the active peripheral portion of nerves exhibited these characters; nor indeed has this fact yet received the assent of many distinguished anatomists. The arrangement of the fine nerve-fibres in the summit of the papillæ of the frog's tongue, described in my last paper presented to the Royal Society (Phil. Trans. June 1864), and in the mucous membrane of the human epiglottis, will, I venture to think, tend to convince many that the really active peripheral portion of the nervous system consists of excessively fine nucleated nerve-fibres arranged as a plexiform network.

With reference to the diameter of these finest branches of the nerve-fibres, many can be demonstrated and followed for long distances which are less than the $\frac{1}{100000}$th of an inch in diameter; and there is reason to think that fibres much finer than this actually exist, and serve as efficient conductors of impressions to and from nerve-centres and peripheral parts.

The Essential Structure of a Nervous Mechanism considered.

Of the supposed terminations of the dark-bordered nerve-fibres, and of the probable existence of nerve-circuits.

It will have been remarked that Continental observers are unanimous in representing the dark-bordered nerve-fibre as passing to its destination unaccompanied by any other fibre whatever. In some drawings it is represented as terminating in a short fine fibre, which is regarded as the prolongation of the axis-cylinder; in others, this fine fibre is represented as bifurcating so as to form two very fine fibres. Some observers consider that the "axis-cylinder" spreads out to form a band which is more or less convoluted, but terminal, forming an "end-organ," while others hold that the fibre gradually ceases or loses itself in the surrounding tissue.

But while there are these minor differences, all agree in the opinion that the nerve-fibre passes alone to its ultimate "end." On the other hand, I have endeavoured to show that at least one fine nerve-fibre accompanies the dark-bordered fibre to its ultimate destination (page 241) as represented in these figures, and that this fine nerve-fibre is a constant structure of great importance. It was first fully described by me in papers in the 'Archives of Medicine' on the frog's bladder; but its existence was referred to in my paper in the Philosophical Transactions for 1860, and its arrangement investigated in that published in the Philosophical Transactions for 1862.

Some surprise may be felt that Continental observers have not specially noticed the fine fibre accompanying the dark-bordered fibre, or referred to my statements concerning it; but as neither the fine fibre, nor indeed the finest part of the dark-bordered fibre, can be seen in specimens examined in aqueous fluids, it was scarcely to be expected that the facts I have described should have been verified in Germany. I therefore beg to direct the attention of anatomists and physiologists to the drawings to which I now point, and to my specimens.

The fine fibre accompanies the dark-bordered fibres distributed to the tissues of the frog generally, but it is more easily demonstrated in relation with the nerves distributed to the bladder, to the mucous membrane of the palate, the skin, and the connective tissue about the heart and lungs, than with those of striped muscle; it is, however, so frequently seen in the case of muscular nerves, especially in the mylohyoid of the Hyla, that I believe it is invariably present, though it cannot be *demonstrated* in all cases. Not only is the structure of the fibre very delicate, but it is often obscured by the dark-bordered fibre which it accompanies.

Now, if a single fibre passed at once to its destination, as Continental observers suppose, it is obvious that the arrangement of the nerves in muscle must be different in principle to their arrangement in the cornea, for example, where it is admitted no "end-fibres" can be detected. But, on the other hand, as at least two fibres, and usually several, pass together to

their ultimate destination, there is at least a possibility, if not a reasonable probability, that the ultimate arrangement of nerve-fibres in muscle, the cornea, and other tissues is, in principle at least, the same. It may be remarked, further, that it is not likely that the nerve-current would be running in the same direction in two distinct fibres situated close together, while the existing anatomical arrangement above referred to is suggestive of currents passing in opposite directions. This view is favoured by the fact of one fibre being much finer than the other—an arrangement which would be fully explained if each of the two fibres were a part of two different circuits. My meaning will be understood at once if this diagram, to which I now point, be examined.

I have endeavoured to prove that in various forms of muscular tissue, and in other textures, nothing but *continuous* nerve-fibres can be observed. The most careful observation has failed to show any appearance which could be considered as demonstrative of "*ends*" of any kind; and although in many cases I have been unable to follow the very fine fibres resulting from the division of the nerve-fibres of one trunk into those of another trunk—although, therefore, there is, as it were, a hiatus in the evidence advanced in favour of this view being universally applicable, the appearances in every one of the cases that I have examined are such as to render it almost certain that this is the real arrangement. If we find a compound nerve-trunk passing to one part of a muscle and another compound trunk passing away from another part of the muscle, in such manner as would be easily explained upon the supposition that certain of the fibres of one cord were continuous with those of the other—more especially if the action of these fibres could be explained upon such an hypothesis, we should surely be justified in inferring the continuity of the fibres, although we could not trace them through their entire course. It might be urged by an objector, that it is just at this intermediate point in many instances that the evidence fails. But it must be borne in mind that it fails in certain instances only; for I have traced and can demonstrate, in some of my specimens, the nerve-fibres distributed to muscular tissue in every part of their course. The truth of my statements upon this anatomical question is in fact admitted in the case of certain muscles; and those who still maintain that nerve-fibres "*end*" in voluntary muscle must maintain that there are some muscles in which nerves form networks, while in others they terminate in distinct ends—that in fact nerve-fibres are distributed to different kinds of muscular tissue upon at least two very distinct principles, although no differences whatever can be shown in the essential structure or action either of the muscular or of the nervous tissue.

But the case is still stronger than this. I shall adduce a considerable amount of collateral evidence in favour of the view that nerves form continuous and uninterrupted cords; and this evidence will be derived from many different sources.

As there is the greatest difference of opinion with regard to the arrange-

ment of the nerves in muscle, and as the question is now much involved, it seems to me of the utmost importance to consider it from a general point of view. Every careful examination that I have made with the view of ascertaining the arrangement of the nerve-fibres in various tissues has tended to confirm me in the opinion that networks and continuous circuits exist, and that there are no "ends" or "terminal extremities." Although I am of course ready to admit that no amount of argument from general considerations can upset the conclusions resulting from direct observation in special cases, I submit that the conclusions of my opponents, in the particular instances advanced by them, have never been supported by positive demonstration. Indications of the appearances they have described undoubtedly exist; but it seems very difficult to prepare specimens which shall admit of but one interpretation, and so distinct that several independent observers would, upon examination, arrive at one and the same conclusion. It is too often urged that the specimens demonstrating "ends" and "end-organs" do not "keep," and must be examined when quite fresh, while I find no difficulty in preserving those which demonstrate "networks" and "plexuses." I desire, however, to weigh carefully every kind of evidence that can be brought to bear upon the determination of this point, which is undoubtedly one of very great difficulty. As the question, too, is a fundamental one of the utmost importance, it is worthy of the most patient consideration.

Of terminal plexuses and networks of fine nerve-fibres in the cornea and in connective tissue.

From its transparency, the simplicity of its structure, and the absence of vessels over at least a great part of its extent, the cornea of the smaller lower animals presents many advantages for studying the arrangement of the ultimate nerve-fibres. My friend and former pupil, Prof. Ciaccio, now of Naples, very carefully studied this subject; and the results of his observations will be found in the Transactions of the Microscopical Society for July 1863, "On the Nerves of the Cornea, and of their distribution in the Corneal Tissue of Man and Animals," by Prof. G. V. Ciaccio, M.D., of Naples. Of the existence of nerve-networks in this tissue there can be no question; but there is some difference of opinion regarding the manner in which the ultimate nerve-fibres are arranged. This drawing represents the nerve-fibres in the cornea of the Hyla. The relation of the finest nerve-fibres to the corneal corpuscles is a question of great importance. Kühne has endeavoured to prove that the ultimate nerve-fibres are continuous with the processes of the connective-tissue-corpuscles, and that there is an actual continuity of tissue, such as he believes exists between the nerve-fibre which perforates the sarcolemma of muscle and the protoplasmic matter which is in actual contact with the contractile tissue.

Careful observation, with the aid of the $\frac{1}{25}$ and $\frac{1}{50}$-object-glasses, has convinced me that there is no such arrangement as Kühne supposes, but

that the nerve-fibres pass over or under the prolongations from the corneal corpuscles without being continuous with them. The fundamental arrangement here seems to be the same as elsewhere. The nerve-fibres run amongst the tissue, but they are continuous neither with the proper fibrous tissue of the cornea, nor with its nuclei; and if any influence is exerted by the nerve upon the tissue or upon the nuclei, it is probably effected by the current which is transmitted by the fibre, and is not due to any direct continuity of texture.

The figure to which I now point, represents a thin layer of the connective tissue covering the posterior part of the mylohyoid muscle of the Hyla, with the nerves and vessels. The bundles of fine dark-bordered fibres can be very readily distinguished from the fine fibres given off from them and forming a very extensive network in every part of the tissue. In this specimen, fibres can be traced from the nerve-trunks to the capillaries, as well as to the nerve-network of fine fibres imbedded in the connective tissue. If the reader imagines muscular fibres placed in the meshes of this network, he will, I believe, have a correct idea of the manner in which nerve is distributed to muscle. (See figure on opposite page.) The same facts are demonstrated in my specimens of connective tissue from the abdominal cavity of the frog, the outer surface of the lungs, &c.

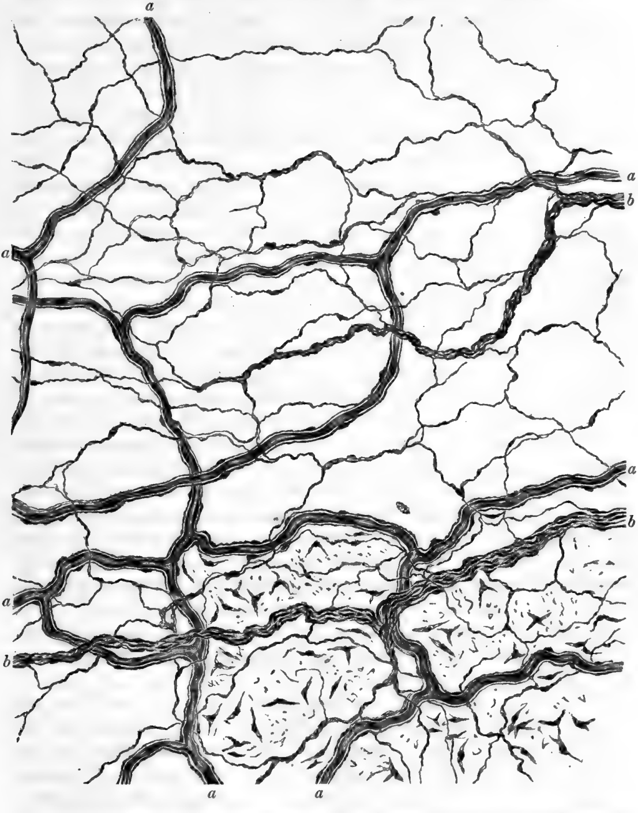
The distribution of the finest nerve-fibres to the mucous membrane of the epiglottis of the human subject is also upon the same plan, but the finest nerves are more difficult to demonstrate. In this figure the capillary vessels and the nerves, as they lie immediately beneath the epithelium, are represented; and in this one a small portion of the tissue cut exceedingly thin, from one of the intervals between the capillaries, is represented magnified by the $\frac{1}{25}$.

Fine nerve-fibres distributed to capillaries in the form of networks and plexuses.

It has been already shown that fine nerve-fibres are distributed to the cornea, to the fibrous tissue in the abdomen of the frog, and to that of the pericardium and of other parts, which is destitute, or nearly destitute, of vessels, and which at the same time is a tissue which can scarcely be regarded as being more immediately influenced by nerve-fibres than the ordinary forms of cartilage, which are undoubtedly destitute of them. To assert that these fibres are in some manner directly concerned with the *nutritive process* is begging the question; and as cartilage is undoubtedly developed and nourished without the direct influence of nerve-fibres, it is probable that the nutrition and development of such tissues as the above, which are closely allied to cartilage, do not depend upon the nerve-fibres which are distributed to them. These nerve-fibres probably perform a totally different service.

Pale nucleated nerve-fibres are also distributed to capillary vessels, as is well shown in the figures to which I now direct attention. That the fibres

seen in the specimens from which these drawings have been taken are true nerve-fibres, is proved by the circumstance of their having been followed from or into undoubted nerve-trunks. The evidence I have adduced in



Connective tissue covering part of the mylohyoid muscle of the frog, and extending from its posterior portion. *a*. Capillary vessels, with their nerve-fibres. *b*. Bundles of fine dark-bordered nerve-fibres, from which fine nerve-fibres may be traced to the capillaries, and to their distribution in the connective tissue, where they form networks of exceedingly fine compound fibres. The engraving represents the specimen as magnified only 110 diameters; but the original drawing was taken from it when magnified by a much higher power.

favour of this view, is of the same nature as that which is admitted to prove

the nervous nature of the fibres distributed to muscle itself; indeed, if these fibres distributed to the capillaries are not nerve-fibres, none of the fine fibres in the cornea, fibrous tissue, &c. already alluded to and represented in my drawings, are nervous. Fine nerve-fibres can be followed from the nerve-trunk, and traced to their distribution on capillary vessels, as represented in this drawing, and as I have also shown to be the case in my Memoir "On the Papillæ of the Frog's Tongue," presented to the Royal Society in June 1864. Some of the fibres can be traced from the immediate neighbourhood of the capillary, where they for the most part ramify, in the surrounding tissue, and may be followed to the point where they pass into undoubted nerve-trunks.

With reference to the office performed by these nerve-fibres, a careful consideration of all the facts I can ascertain in connexion with this question, leads me to the conclusion that these fibres, close to the capillary vessels and in tissues destitute of capillaries, are not concerned in special sensation, but are the afferent fibres to the nerve-centres in which the efferent fibres distributed to the small arteries take their rise. I believe that these fibres do exert an influence upon the process of nutrition, but only by their indirect influence upon the nerves which govern the calibre of the small arteries transmitting the nutrient fluid to the capillaries nearest to the tissues in which they ramify.

Although time precludes me from entering into this part of the inquiry, I may be permitted to allude briefly to the mechanism which I believe is concerned in regulating the nutritive process, as it occurs in the tissues of man and those higher animals whose nutritive operations continue to be carried on with comparatively little alteration under very varying external conditions. The arrangement I am about to describe appears to be, within a certain range of variation, *self-adjusting.* If, however, the limits be overstepped in either direction, as not unfrequently happens, under the very artificial conditions to which man and many of the domestic animals are exposed, the range of self-adjustment is exceeded, and oftentimes a part of the mechanism is completely destroyed and can never again be effectually repaired or replaced.

It is obvious that the afferent fibres above referred to, must be affected by any alterations occurring in the flow of pabulum to the tissue in their immediate neighbourhood. Suppose, for example, the quantity of nutrient pabulum flowing to the cells of a tissue to which nerve-fibres of this class are distributed, to be unusually great, these nerve-fibres would necessarily be compressed by the swelling of the surrounding elementary parts which absorb the pabulum. This pressure would, in the first instance, so affect the nervous centre as to cause a change in the condition of the efferent nerve-fibres, which would induce contraction of the small arteries transmitting the blood to the capillary vessels, and thus the quantity of pabulum sent to this locality would be immediately reduced. The nuclei of the nerve-fibres would also participate in the increased absorption of nutrient

matter; but precisely in what manner, I must not now discuss. If, however, the conditions which led in the first instance to the increased nutrition persisted, the pressure upon the nerve-fibres might go to the extent of paralyzing them, in which case the small arteries would become dilated; the capillaries must in consequence be fully distended with blood, and that congestion which constitutes one of the earliest stages of inflammation as it occurs in man and the higher animals, would result.

I have already indicated the wide differences in structure, mode of growth, and in the changes occurring during action, between the spherical and oval nerve-cells, and the so-called caudate nerve-cells. These differences are sufficiently marked to justify me in regarding them as two distinct classes of central nerve-cells performing very different offices or functions*.

Several considerations have led me to conclude that the oval and spherical ganglion-cells are the sources of nervous power, while the so-called caudate nerve-cells in the cerebro-spinal centre are the points at which several different nerve-circuits intersect, and probably act and react upon one another. The marvellously complex and combined nervous actions depend, in all probability, upon the perfection attained by this latter part of the nervous mechanism. I have been led further to the opinion, not only that the spherical and caudate nerve-cells are concerned with the reflex phenomena of the vascular system, but that those forming the ganglia on the posterior roots of the spinal nerves are intimately connected with the general reflex actions occurring in the voluntary muscles when the cord is divided transversely. From the arrangement of the vascular nerves distributed to the vessels of muscles, it is easy to understand how, by an increased action of these vascular nerves, the contraction of the muscles of a limb might be caused. I have demonstrated that connexions exist between the peripheral portion of purely sensitive nerves and the nerve-fibres distributed to the tissues in which capillaries ramify, as well as to capillary vessels themselves. These connexions would account for the excitation of involuntary reflex actions by the application of a stimulus to the general cutaneous surface. If this view be correct, the ganglia on the posterior roots of the nerves, rather than the different segments of the spinal cord, must be regarded as the centres of reflex actions and also as the nervous centres which, with the so-called sympathetic ganglia, preside over all the vascular, and, through the vessels, over the nutritive phenomena of the body. The facts and arguments in favour of these general conclusions will form the subject of a separate memoir.

Arguments in favour of uninterrupted circuits, deduced from an examination of the trunks of nerves, and arrangement of nerve-centres.

One is somewhat surprised that the mode of branching of nerves, referred to generally in pp. 239, 240, which is so universal, has not been dwelt upon

* See my paper "On the Apolar, Unipolar, and Bipolar Nerve-cells," &c., Phil. Trans. 1863, and a paper entitled "Indications of the Paths taken by the Nerve-currents," &c., Proceedings of the Royal Society, vol. xiii. p. 386.

and carefully described by those who have written upon the structure and arrangement of nerves. The nerves distributed to a tissue or organ are often represented as if they all passed straight to their terminal distribution, while the invariable arrangement is such as to lead to the inference that, of the fibres composing a bundle of nerves, some are proceeding in a direction *from,* and others *towards* the nerve-centre or peripheral part; and this is observed not only in purely motor, but in purely sensitive, as well as in mixed nerves. It is also found in the case of the sympathetic system, and is to be demonstrated in all animals. It is, however, not possible to dissect the trunk of a fine nerve and render it sufficiently transparent to display these facts, if the ordinary methods of examination be adopted; but by the plan of investigation I have fully described, the arrangement may be readily demonstrated in the nerves either of the higher or lower animals, although with the greatest facility in the Hyla*. Few anatomical facts seem to me of more interest and importance in their general bearing upon the physiology of the nervous system than that above alluded to. Its constancy proves its importance, if it does not alone compel us to infer that it is essential. What explanation, then, can be offered of the three sets of nerve-fibres which can invariably be traced at the point where a nerve-fibre comes off from a trunk passing at right angles to it, as represented in these figures? Look at it how we may, there must be three sets of fibres in all cases; and just as we find that the nerve-fibres constituting the roots of the nerves divide soon after their entrance in the spinal cord into bundles which pursue many different directions—some passing upwards towards the brain, others downwards towards the lower segments of the cord, and some to the opposite side, as has been well shown by the researches of Lockhart Clarke—so in the case of every nerve-fibre which appears to pass into or come from an adjacent nerve-trunk, fibres pursue three different courses, as shown in these drawings.

These may be *afferent, efferent,* and *commissural*; and there are fibres commissural as respects different parts of the peripheral system as well as of the central organ. Thus, I believe, may be explained the action of each papilla as a separate organ, independently of its neighbours, or the harmonized action of several different papillæ. By the same arrangement I consider the harmonious action of the several elementary fibres entering into the formation of muscle is effected.

As has been before observed, the large compound nerve-cords or trunks, the finer bundles, and the finest constituent fibres of the pale terminal nerve-fibres exhibit the same general arrangement. The remarks already made with reference to the course of the fibres in the nerve-trunks and the branching of the dark-bordered fibres, also apply to the finest fibres; and at the point where a fibre passes off from another at right angles, the existence of the three sets of fibres can be demonstrated. I would draw

* How to work with the Microscope. Third Edition, p. 204. See also "On the Branching of Nerve-trunks," &c., Archives, vol. iv. p. 127.

attention to the arrangement shown in these drawings, and especially to that represented in this figure; not that I would maintain that in the finest fibres these three fibres are separated from one another or insulated by a layer of white substance, but, on the contrary, I consider that in many cases these fine fibres, although they may often be split in the longitudinal direction, nevertheless in their natural state form almost homogeneous fibres, the material of which may permit the passage of nerve-currents in the different directions indicated. It is very probable that the passage of the currents along precisely the same paths for a considerable time may cause the decomposition of the nervous matter in such a manner as to give rise to distinct lines, which might readily be mistaken for separate fibres, and after a time lines of fibres in an apparently transparent tissue would result*. At an early period of development, nerves form a sort of thin expansion, in which the appearance of fibres crossing one another in various directions may be afterwards produced by the passage of the nerve-currents. Beneath the external investment of the common fly and many other insects, and beneath the soft, delicate perivisceral membrane of mollusca, I have seen the most beautiful and elaborate arrangement of apparent nerve-fibres of such a character as to justify the above inference.

In the cornea, that part of some of the nerve-fibres from which several fine bundles radiate in different directions exhibits lines or fibres crossing one another in every direction which the emerging fibres take.

This subject is capable of much further elucidation, and is well worthy of being considered in detail; but in this Lecture I only allude to it cursorily because it bears, in a most important manner, upon the question of uninterrupted nervous circuits, and affords an explanation of the manner in which the complex arrangement which nerves ultimately exhibits is brought about.

Of the "termination" of nerves in papillæ and in special cutaneous nervous organs, such as the papillæ concerned in touch and taste, and in the Pacinian corpuscles.

Now in highly elaborate nervous organs like the papillæ of the frog's tongue, which are very minute, and situated comparatively close to one another, we have an opportunity of studying, under great advantages, the course pursued by the constituent fibres of a bundle of nerves. And although even here it is not possible to follow a single fibre for any great distance, a careful consideration of what can be demonstrated leads to the inference that to every one of these papillæ three sets of nerve-fibres are distributed.

I have always been able to demonstrate in the peripheral organs that I have examined more than a single nerve-fibre; and where, as is almost

* Indications of the Paths taken by the Nerve-currents, &c.: Churchill and Sons.

invariably the case, numerous terminal organs exist, these are always connected together by nerve-fibres which pass from one to the other. Although the arrangement is not always so distinct as represented in this drawing of the papillæ from the tongue of the frog, I always find that where a nerve-trunk divides into two sets of branches, there exists at the point of division a fibre or fibres which seem to connect the two terminal organs to which the bundles of fibres pass. Passing to every touch-body in the papillæ of the skin of the finger, I find more than one nerve-fibre; and the corpuscle itself seems to consist of a very much coiled and reduplicated nucleated nerve-fibre, as represented in this drawing.

In the peripheral cutaneous nervous organs of many invertebrate animals which I have examined, especially in some of the insects and annelids, I find a *bundle* of nerve-fibres, not a *single nerve-fibre*, as is usually represented. This drawing illustrates the view generally entertained; and this one, my own inference of the structure of these organs. Even in the Pacinian body I find no such indications of a true *termination* of the axis-cylinder as is usually described: not only so, but in many cases I have seen three or four very wide lobe-like continuations of the axis-cylinder bending downwards from its highest point, and passing apparently into very fine granular fibres which lie between the laminated capsules, and are continued into the nerve-sheath. The drawing will illustrate the structure of all these allied peripheral nerve-organs, according to my observations.

Evidence in favour of continuous nerve-circuits, derived from the study of the development of nerve-fibres distributed to muscle.

The development of nerves distributed to muscle is most difficult to investigate, but it is a subject well worthy of most attentive study; and although I cannot hope to give a clear account of the process, I shall make an attempt to describe what I have myself seen. The relation of nerves to the contractile tissue of muscle and other tissues, and the general arrangement of nerve-fibres, having been determined, one cannot avoid asking how the fibres became arranged as we see them in the fully formed texture.

Of the part taken by the masses of germinal matter there cannot be the slightest doubt; for it can be shown most conclusively that as the nerves advance from the early to the complete stage of their development, the distance between the several masses of germinal matter gradually increases. This may be proved in the case of dark-bordered as well as of very fine nerve-fibres. It is well shown in these figures. At an early period of the development of muscle, very numerous masses of germinal matter are seen amongst the muscular fibres, in which transverse markings are already developed. These, as I have been able to satisfy myself by researches upon the diaphragm and intercostal muscles of the fœtal dog, are concerned in the formation of nerves and capillaries.

In the young caterpillar the surface of some muscular fibres seems to be

completely covered with nuclei; and as development advances these nuclei or masses of germinal matter seem to separate further and further from one another, and the delicate nerve-fibres might be said to be drawn out from them. At the same time the muscular fibre increases in size. It will probably be conceded that at an early period of development of a muscle there are masses of germinal matter taking part in the development of the three different structures—*muscle, nerves,* and *vessels.* Besides these, upon the surface of the muscle, and between the muscular fibres, are masses which have perhaps already given rise to the formation of a soft granular and slightly fibrous connective tissue. I think that these last masses have originated from the same parent masses as the others. Indeed it is certain that this must be so. Of the masses taking part in the development of a bundle of nerve-fibres, those on the surface produce not true nerves, but connective tissue, and so with regard to muscles, vessels, and other textures.

The part taken by the germinal matter in the development of muscles, nerves, and vessels may be studied in the fully-formed frog, and with greater facility than in the embryo. At certain intervals amongst the large muscular fibres of the frog may be discovered with some difficulty some bundles of finer muscular fibres. These are most distinctly seen, however, in the thin breast-muscle of the frog, where they were discovered by Kölliker. They were termed by him "*nerve-tufts,*" and are figured in his *Croonian Lecture,* delivered in 1862 (Proceedings of the Royal Society, 1862, p. 78). I have had Kölliker's figure copied. It does not, however, represent all that may be seen in these swellings, prepared according to the particular plan before alluded to (p. 258); for the numerous oval nuclei, figured in Kölliker's drawings, are represented by him as being pretty generally diffused throughout the swelling, and as not being connected with one another, or with any definite structure. The relation of the nerve-fibres to these nuclei is not indicated in Kölliker's drawing, nor is the meaning of these numerous nuclei discussed.

Some of the nuclei (masses of germinal matter), however, are seen in my specimens to be nuclei in the course of very fine nerve-fibres—nuclei which take part in the formation of the nerve-fibres themselves. Others are the nuclei of the muscular fibres which are undergoing development, and over the surface of which the fine nerve-fibres are spread out. These facts are demonstrated in several specimens which I have mounted in strong glycerine and acetic-acid syrup*. A portion of one of these is represented in the figure to which I now point. This drawing, which is magnified 700 diameters, appears somewhat confused, owing to the very close proximity of the nerves to the muscles. It is, however, a careful copy of one of my specimens just at the spot where *three* dark-bordered nerve-fibres pass into, or emerge from, one of the "nerve-tufts." In this drawing I have shown one branch of a dark-bordered nerve-fibre and its division into two very fine

* "I have found this strong acetic-acid syrup a most valuable agent in these and kindred investigations."—*How to Work with the Microscope,* third edition, p. 202.

fibres. These may be followed for a considerable distance amongst the developing muscular fibres.

It has been truly stated by Kölliker that the apparently single muscular fibre bearing the swelling is really *a bundle of very fine muscular fibres*, varying from three to seven, or more, in number, and that the apparently *penetrating* nerve-fibres merely pass between these imperfectly developed muscular fibres. I cannot, however, agree with him in the view that the fine muscular fibres result from longitudinal splitting of wider fibres. The bundles of fine muscular fibres under consideration extend, it is true, at a certain period of their development, from one extremity of the muscle to the other; but all the muscular fibres of the bundle do not reach so far. In one bundle sometimes ten or twelve distinct muscular fibres, very closely packed together, may be counted. Near the swelling the muscular fibres are wide, and the fine, tapering, pointed extremities of other young muscular fibres can also be seen. These spindle-shaped muscular fibres are not nearly so long as the ordinary fully developed muscular fibres. In fact, at the swelling, several *spindle-shaped, nucleated*, already *transversely striated* muscular fibres may be observed, and the stages through which the elementary fibres of voluntary muscle pass in their development may be traced.

In these "nerve-tufts" we may indeed study, in the fully-formed animal, striped muscle and nerve in every stage of development. Vessels cannot be traced into the youngest tufts; but in those which consist of several partly grown muscular fibres, capillaries are to be seen; so that the development of muscles, nerves, and vessels can be studied in these imperfectly developed "tufts."

From the above observations, it will be seen that I cannot agree with Kölliker in the view he has taken of these bodies. He says, "Now if it be admitted that the finer muscular fibres composing the bundle are generated by the division of thicker muscular fibres, as Weismann justly concludes, the explanation of the nerve-tufts becomes easy, inasmuch as they may be conceived to arise from *a simultaneous growth and division of the nerve-fibre belonging to the parent muscular fibre, in order that each of the young muscular fibres may obtain its branch of nerve*" (Croonian Lecture, May 1862).

So far from the narrow young muscular fibres *resulting from the division of old ones*, the young muscles and young nerves *are developed from collections of nuclei or masses of germinal matter, precisely resembling those which are found in the embryo*. I believe this to be an invariable law. Many facts make me feel confident that it is quite impossible that *new* textures can be formed by the subdivision of old ones. Formation and development take place upon precisely the same principle in young and old tissues, in health and disease, in simple and complex organisms. New muscular fibres may be developed from old ones in this way:—the "nuclei" may increase in number, the old muscular tissue may *undergo disintegra-*

tion and disappear, in fact the nuclei may live and increase at its expense, and a new mass, consisting entirely of nuclei, or masses of germinal matter, by the agency of which the formed material of the new fibres is at length produced, may result; but never does old tissue split up into new tissue.

As I have pointed out on many occasions, in fully-formed organs there exists a certain proportion of *embryonic germinal matter,* which may undergo development at a future period of life, and if the greater part of this becomes fully-formed tissue, still there remains *embryonic matter for development at a still later period,* and so on. In the situations of these so-called nerve-tufts in the breast-muscle of the frog, new elementary muscular fibres are added to those already formed, and the muscle grows as the frog advances in age. In the formation and growth of the muscular fibres, and in the formation and arrangement of the nerves around them, the movements of the several nuclei or masses of germinal matter to which I have drawn attention, play no unimportant part. (See my paper "On the movements of the living or germinal matter of the tissues of man and the higher animals," Archives, vol. iv. p. 150.)

With reference to the nerves supplying these so-called nerve-tufts, I would remark—

1. That two dark-bordered nerve-fibres, running in the same sheath, may often be traced to one part of the "nerve-tuft."

2. Besides the dark-bordered fibre or fibres, there are invariably very fine fibres running in the same sheath.

3. That the dark-bordered fibres and the accompanying fine fibres divide and subdivide very freely amongst the young muscular fibres, and that thus quite a leash of very fine nerve-fibres results, in the course of which numerous nuclei exist at certain intervals. Many of these can be followed upon or between the muscular fibres, for the distance of the twentieth of an inch or more from the oval swelling. These points are well seen in the figures to which I now direct attention.

4. That the dark-bordered fibre or fibres which enter at the tuft are *not the only* nerve-fibres distributed to these bundles of muscular fibres, but that invariably a bundle, consisting of two or three fine but dark-bordered fibres, is connected with the muscular fibres, at a point above or below that at which the swelling is situated, where the large fibre or fibres enter. Sometimes there are two such bundles, one above and one below. These not unfrequently give off branches, just before they pass to the muscular bundle, which pursue a longer course, and are distributed to other larger muscular fibres; and oftentimes branches pass from one muscular bundle to more distant ones.

From the above observations it follows that these "nerve-tufts" in the breast-muscle of the frog consist of developing muscular fibres, which are freely supplied with nerves; and the number and distribution of the nerves render it probable, not only that there are *entering* and *emerging* fibres, nerve-loops, and plexuses, or networks, upon the muscular fibres, rather

than *free ends*, but that the action of the new muscular fibres may be harmonized with those of the other and older elementary muscular fibres of the muscle by branches of nerve-fibres which are probably commissural.

I will next venture to consider the nature and origin of the nuclei taking part in the development of the muscular nerves; and I would remark that in the frog it is comparatively easy to study the formation of even complex organs out of what used to be called a granular blastema. In each succeeding spring-time not only new ganglion-cells but new ganglia and nerve-fibres, as well as vessels, are developed, and take the place of those which attained their perfect condition in the previous year, but which, having performed their work, have wasted and become converted into mere débris, a great part of which was removed during the period of hybernation.

Now the formation of a new ganglion, of new muscular fibres, of new vessels, and other tissues, and even the formation of elementary organs of complex structure (as I have ascertained specially in the case of the uriniferous tubes of the newt), results from changes taking place in a collection of small spherical masses of germinal matter; and these collections themselves seem to result from the division and subdivision of at most a few masses, all of them of course being the descendants of the original germinal mass formed when impregnation occurred.

Now it may be affirmed most positively, that an entire organ, such as the kidney-tube, or an elementary fibre of muscle, is not formed first and the nerve then spread over it, but the development of the tissue to be influenced proceeds *pari passu* with the development of the nerves which are to influence it. And in the adult animal, where the development of new nerve-fibres takes place, new muscles, &c., are developed in relation with them. I have reason to think, indeed I feel confident, that new nerve-fibres cannot be developed so as to influence an old muscular fibre, or old nerve-fibres caused to influence newly developed muscular tissue; and in the wasting of certain muscles, or other complex tissues, to which nerves are distributed, as may be studied in the frog, all the old tissue seems to be destroyed and removed by the increase of the germinal matter of the respective tissues. Hence it may be stated positively that in every case the new tissue is developed from a mass of "formless blastema"—that is, from a collection of spherical masses of germinal matter which could not be distinguished from the embryonic mass or collection which forms the early condition of every living thing in nature; and in the destruction and removal of every tissue and organ, masses of germinal matter, often resulting from the division of those of the tissue itself, absorb, remove, and in fact live at the expense of, the tissue which is to disappear; and whether this change occurs *physiologically* (that is, as a normal change at certain periods in a healthy and well-developed animal) or *pathologically* (that is, in an organism which has been subjected to the influence of conditions more or less adverse to its well-being), the process is essen-

tially of the same nature; and it would indeed be very difficult to distinguish a collection of spherical masses of germinal matter, from which the tissues of a new being are to be evolved, from a mass of young pus-corpuscles, which may result from the rapid multiplication of masses of germinal matter existing in any tissue of man or the higher animals. In both cases the matter is *formless*; and however much the conclusion may be opposed to the affirmations of great authorities, we are compelled, by a review of the facts ascertained by observation, to infer that there is a far greater difference in the power than there is in the chemical characters, or physical properties, of the matter taking part in these changes.

Many very interesting and highly important facts relating to this inquiry may be obtained from a careful study of the minute changes which occur in the development of the tissues of the imago or perfect insect during the chrysalis stage. So far as I am able to ascertain, the larval tissues and organs are in the first instance completely removed, the germinal matter increases considerably in quantity, and at length a collection of new masses of germinal matter results, which take part in the formation of the new tissues of the developing imago. If those who so confidently affirm that all the phenomena of living beings are physical and chemical would investigate some of these marvellous changes, I venture to think they would very soon withdraw their confident assertions, and admit that the construction of tissues and organs is a process not to be explained by physics and chemistry, or accounted for by any of the known laws of ordinary lifeless matter or force.

I must now advert to a question which I feel incompetent to grapple with, though I cannot permit myself to pass it over. Let me consider if, in the development of new muscular fibres, nerves, and vessels, as occurs in the case of the nerve-tufts of the frog, or in the development of a new ganglion connected with the sympathetic, there are certain masses of germinal matter which, as the direct descendants of pre-existing masses in muscles, nerves, vessels, &c., take part in the development of these tissues respectively, or if they all result from changes occurring in what would be called by some a mass of undifferentiated blastema? In studying the early developmental changes taking place in the embryo, one discovers nothing which would justify the inference that one set of masses is concerned in the development of *all* the future muscles of the body, another of *all* the vessels, another of *all* the nerves, another of all the glandular organs, and so on,—each of these masses or collections being gradually prolonged to distant parts; but it seems rather that the whole is in the first instance formless, and that the process of formation gradually proceeds in many parts at the same time. The brain is not formed first, and other parts of the nervous system extended from this central organ; but the active nervous system, central and peripheral, is developed as a whole, stage succeeding stage, until it attains its fully developed condition in all its parts. If masses of germinal matter for the development of the respective tissues were first formed, and

an extension from each of these to distant parts took place, it must follow that the portion first formed would be the oldest; but all observation seems to show that development gradually goes on in different and distant parts at the same time. And I infer that in the process of regeneration of the lobster's claw, or of the lizard's tail, of the fully formed animal, the several tissues constituting the organs are entirely developed anew from a formless mass, and not by the simple extension of the tissue of the muscles, nerves, vessels, &c., which exist in the stump. In the first instance there results a soft material, which exhibits no indications of definite structure; and as development proceeds, the masses of germinal matter taking part in the development of nerves are seen arranged in lines, and are continuous with those in the nerves of the stump. It is, however, possible that new masses of germinal matter may grow and multiply from these latter and extend into the soft indefinite tissue first produced and destined to serve only a very temporary purpose; but, before I can consider this question advantageously, I must make further observations. And it appears from observations in the case of the frog, that when a new peripheral part or organ is developed, new central nerve-cells are developed in connexion with it. And it is probable (indeed it appears to me certain) that even in man this development of new central and peripheral organs goes on in certain instances. For example, at each pregnancy in the human female, it is probable not only that new muscular fibres, vessels, nerves, &c. are developed in connexion with the growing uterus, but that new *nerve-centres* are also produced, with which the new nerves are connected; and I regard it as most probable that during the development of the lizard's tail and lobster's claw new central nerve-cells in connexion with the new nerve-fibres are developed in the already existing but comparatively simple nerve-centres.

Of the relation of the ultimate branches of the nerve-fibres to the elements of the tissue and to the germinal matter.

In no case does the nerve become continuous with any part of the contractile tissue of muscle; nor is it connected with the nucleus of the muscular fibre or with that of any other tissue.

The ultimate nerve-fibre bears the same relation to the contractile tissue of muscle that it bears to fibres of white fibrous tissue, to cells generally, and to the processes of cells, such as the prolongation from the pigment-cells of the frog, those of the corneal corpuscles in the cornea, &c. The arrangement is such as would lead us to infer that the tissue is influenced by the current passing through the nerve, not by any change involving an anatomical continuity of structure from the nerve to the tissue affected by it, or even in actual contact with any part of it; for in very many instances we can prove that the nerve is not in very close contact with the tissue it influences. Moreover, results resembling those which occur from the action of a nerve may be brought about by the passage of a current of electricity through a wire situated at a considerable distance from the muscle, and

separated from it by non-conducting media; so that, as I have before mentioned, it would seem probable that the varying degrees of muscular contraction are induced by the varying intensity of the current transmitted along a continuous nerve-fibre.

Arguments in favour of the existence of continuous nervous circuits founded upon the structure and arrangement of ganglion-cells.

In a paper already referred to, communicated to the Royal Society in 1864, and published in the 'Transactions,' I endeavoured to show that certain ganglion-cells which had been considered to be apolar or unipolar were invariably connected with at least two nerve-fibres, and that in many cases one of these fibres was coiled spirally round the other, as is well shown in this drawing. These two fibres often appear as one; but not only have I succeeded in demonstrating that they are derived from *different parts of the same cell,* but that they pursue *opposite directions* in the nerve-trunks. I have been led to conclude that all nerve-cells give origin to more than one nerve-fibre, and that these fibres, although they run parallel to one another for a short distance, diverge and pursue very different and indeed opposite courses; and I endeavoured to show that the arrangements I had observed received a ready explanation upon the view of the existence of complete nervous circuits.

In another communication previously referred to, published in the 'Proceedings' of the Royal Society for 1864, entitled "Indications of the paths taken by the nerve-currents as they traverse the caudate nerve-cells," I showed that there existed in the caudate nerve-cells of the spinal cord and medulla oblongata a remarkable series of lines, which passed from each fibre connected with the cell across the body of the cell into every other fibre which diverged from it. I regarded these as indications of the paths taken by the nerve-currents which traversed these cells, and my observations led to the inference that every single cell was the seat of decussation, and therefore formed part of the course, of a vast number of *different nervous circuits.* Upon this view of the constitution of the highly complex central organs of the nervous system, it is not difficult to account for the marvellous number of distinct actions effected, or of the still more wonderful combinations of actions which must occur in the great central organs of the nervous system of man and the higher animals. The axis-cylinder of each dark-bordered nerve-fibre probably forms the common route along which nerve-currents pass from many different parts in the nerve-centre to as many different points in the periphery. Fibres prolonged from several different nerve-cells seem to combine to form one dark-bordered fibre; but these and other points will be readily understood by a cursory examination of the diagrams to which I now direct attention, so that it is unnecessary for me to describe them minutely.

General Conclusions.

To sum up briefly the results of this prolonged inquiry. The first import-

ant point is, that in no tissue have I been able to demonstrate an '*end*' to a nerve. In all cases the nerve-cell or nucleus exhibits fibres proceeding from it in at least *two opposite directions*. The apparent cessation or thinning off of the nerve-fibre in many tissues results from its becoming so thin as to be invisible, unless special methods of investigation are resorted to. It has also been shown that near nervous centres, and near their peripheral distribution, the bundles of nerve-fibres and the individual nerve-fibres divide into very numerous branches. The bundles of coarse or fine fibres given off from a large or small trunk consist of fibres which pursue opposite directions in that trunk, one set passing as it were *from*, the other *towards*, the nervous centre. The nerves distributed to striped muscle of all kinds and to the various forms of unstriped muscle in vertebrata and in invertebrata, are arranged so as to form networks and plexuses, but no indication of terminations or ends is to be seen.

These facts seem to render it probable that the fundamental arrangement of a nervous apparatus is a complete and uninterrupted circuit. This view is supported by the existence of at least two nerve-fibres in all peripheral organs and by facts observed in the branching and division of individual nerve-fibres and of compound nerve-trunks. I have also shown that in nerve-centres it is doubtful if apolar or unipolar cells ever exist. All nerve-cells have at least two fibres proceeding from them in opposite directions, and the multipolar cells in the brain and cord exhibit lines across them which are probable indications of the paths taken by continuous currents which traverse them in many different directions.

The general inference from this anatomical inquiry is, that a current probably of electricity is constantly passing through all nerve-fibres, and that the adjacent tissues are influenced by the varying intensity of this nerve-current rather than by its complete interruption and reestablishment; so far as I know, no fact has ever been discovered which would justify the conclusion that there exists any arrangement for making and breaking contact in any part of the nervous system. In all cases it is probable that every nervous circuit is complete, and that there is no interruption of the structural continuity of a nerve-fibre at any part of its course.

May 18, 1865.

Major-General SABINE, President, in the Chair.

His Royal Highness Louis Philippe of Orleans, Count of Paris, was admitted into the Society.

The following communications were read :—

I. "On Newton's Rule for the Discovery of Imaginary Roots of Equations." By J. J. SYLVESTER, F.R.S. Received May 4, 1865.

In the first part of my "Trilogy of Algebraical Researches," printed in

the Philosophical Transactions, will be found a proof of Newton's Rule for the discovery of imaginary roots carried as far as equations of the 5th degree inclusive. The method, however, therein employed offered no prospect of success as applied to equations of the higher degrees. I take this opportunity, therefore, of announcing that I have recently hit upon a more refined and subtle method and idea, by means of which the demonstration has been already extended to the 6th degree, and which lends itself with equal readiness to equations of all degrees. Ere long I trust to be able to lay before the Society a complete and universal proof of this rule —so long the wonder and opprobrium of algebraists. For the present I content myself with stating that the new method consists essentially, first, in the discerption of the question as applied to an equation of any specified degree into distinct cases, corresponding to the various combinations of signs that can be attached to the coefficients; secondly, in the application of the fecund principle of variation of constants, laid down in the third part of my 'Trilogy,' and, in particular, of the theorem that if a rational function of a variable undergoes a continuous variation flowing in one direction through any prescribed channel, then at the moment when it is on the point of losing real roots, not only must it possess two equal roots (a fact familiar to mathematicians as the light of day), but also its second differential, and the variation, when for the variable is substituted the value of such equal roots, must assume the same algebraical sign*. By aid of the processes afforded by this principle, which admits of an infinite variety of modes of application, according to the form imparted to the channel of variation, and constitutes in effect for the examination of algebraical forms an instrument of analysis as powerful as the microscope for objects of natural history, or the blowpipe for those of chemical research, the problem in view is resolved with a surprising degree of simplicity; so much so that, as far as I have hitherto proceeded with the inquiry, the computations, algebraical and arithmetical, which I have had occasion to employ may be contained within the compass of a single line. The new method, moreover, enjoys the prerogative of yielding a proof of the theorem in the complete form in which it came from the hands of its author (but which has been totally lost sight of by all writers, without exception, who have subsequently handled the question), viz. in combination with, and as supplemental to, the Rule of Descartes. On my mind the internal evidence is now forcible that Newton was in possession of a proof of this theorem (a point which he has left in doubt and which has often been called into question), and that, by singular good fortune, whilst I have been enabled to unriddle the secret which has baffled the efforts of mathematicians to discover during the last two centuries, I have struck into the very path which Newton himself followed to arrive at his conclusions.

* The above is on the supposition that there is no ternary or higher group of equal roots.

Received May 18th, 1865.

Since the above note was sent in to the Society, I have completed the demonstration for the 7th degree, and in the course of the inquiry have had occasion to consider the conditions to be satisfied in order that a rational function of x, with r equal roots a, may undergo no loss of real roots for any assigned variation imparted to the function: for the theory of the 7th degree the case of three equal roots has to be considered, and the conditions in question are that the variation itself may contain the equal root a, and that its first differential coefficient may have the contrary sign to that of the third differential coefficient of the function which it varies when a is substituted for x—a theorem which is, of course, capable of extension to the case of an equation passing through a phase of any number of equal roots*.

II. "On the Application of Physiological Tests for certain Organic Poisons, and especially Digitaline." By C. Hilton Fagge, M.D., and Thomas Stevenson, M.D. Communicated by J. Hilton, F.R.S. Received May 4, 1865.

(Abstract.)

As the chemical processes for the detection of certain organic poisons are very inconclusive in their nature, and as many of these agents produce effects of a most remarkable kind on the lower animals, it is not surprising that their physiological action should have been employed as a test for their presence. Thus Dr. Marshall Hall suggested as a means of discovering strychnia, the tetanic symptoms which that alkaloid causes in frogs; and quite recently MM. Tardieu and Roussin produced a large mass of physiological evidence, in a French "cause célèbre", in which digitaline was believed to be the poison used.

Those who have recommended the employment of evidence of this nature have always relied on the similarity between the symptoms observed in the case of supposed poisoning during life, and the effects obtained on the lower animals by the extract believed to contain the toxic agent; and as the action of poisons on man and on the lower vertebrata is certainly not always the same, the value of these physiological tests has been much disputed, and is not now admitted by most authorities in this country. It appears to us, however, that physiological evidence may be made independent of any relation of this kind. It is sufficient that the action of the

* The above is on the supposition that one of the three equal roots remains unaffected in magnitude by the variation, whilst the other two change. If all three are to change simultaneously, infinitesimals beyond the first order and with fractional indices have to be brought into consideration; in that case, on making $x=a$, the variation need not become absolutely zero, but must contain no infinitesimal of the first order. And a further limitation becomes necessary in addition to the conditions stated in the text, in order that no loss of real roots may be incurred in consequence of the variation.

substance believed to contain the poison on the animal experimented on be identical with the known effects of that poison *upon the same animal*, and that these effects be capable of being produced by no other agent or, at any rate, only by a limited number of other agents.

In this spirit we have conducted a series of investigations, with reference to the detection of digitaline and of certain allied substances. We selected that poison, not only because of the interest which attaches to it at the present time, but also because the chemical tests for it are peculiarly inadequate. The animals which we employed in all our experiments were frogs. Their sensibility to small quantities of poison, the fact that they are but little liable to be affected by fear or other accidental circumstances, and the independence of their organs, which makes it possible to determine with accuracy the nature of the effects produced, have rendered them better adapted for this purpose than any other animals; and the objection ordinarily urged against their use, that the action of poisons on them is often different from that of the same substances on the higher animals, has no validity when the question of physiological evidence is looked at from our point of view.

It has been expressly denied, by those who have advocated the use of physiological tests, that animal extracts, such as those obtained from the contents of the human stomach, or from vomited fluids, could in themselves be poisonous to the lower animals. We thought it desirable, however, to make some direct experiments upon this point; and, to our surprise, we found that in almost every instance the toxic action of such extracts was most decided and unmistakeable. The effects produced were indeed very different from those caused by digitaline; and we think that we have been able to distinguish quite clearly between them. Still, the recognition of the fact that these extracts exert a poisonous action, independently of the presence of any of the ordinary toxic agents, must have an important bearing upon the application of physiological evidence. Unless some points of difference should hereafter be discovered, it will render impossible the detection of many vegetable substances (among which we may mention lobelia, emetina, veratrum viride, and delphinium staphisagria) by their physiological effects. And it makes invalid (at least so far as frogs are concerned) all evidence of this kind, in which the state of the heart is not more particularly described than has hitherto been the case, so far as the frog-test for strychnia is concerned; on the other hand, though this was not the primary object of our inquiries, we may remark that tetanic spasms were produced by none of the numerous substances with which we experimented, except veratrine and theine. It is of course well known that other agents, and notably some of the constituents of opium, produce tetanus in frogs; but on the whole our experiments lead us to hope that this test will hereafter be found of more value than is now generally supposed to be the case.

We have devoted a considerable number of experiments to the solution

of the practical question, whether it be possible to obtain the characteristic effects of digitaline, not only from the extracts of liquids to which it had been artificially added, but also from extracts of the stomach-contents and vomited matters of dogs poisoned by that substance. The results of these experiments were perfectly satisfactory; and we think that our observations show conclusively that there is no difficulty in obtaining from these complex mixtures physiological effects identical with those of a pure solution of digitaline.

Far more difficult to decide than the question of practical applicability, is the question as to the theoretical accuracy and conclusiveness of the physiological test for digitaline and the allied poisons. To this question we do not venture to give a positive answer. Our experiments justify, as we think, the hope that this test will be hereafter found of very considerable value in aiding in the detection of these substances; but it can be only by the combined labours of many observers, and not merely by one series of experiments, that this point can be finally settled.

The following are the conclusions at which we have arrived, and which are deduced from our own experiments in every instance, except where the contrary is expressly stated, under heading 2.

1. Digitaline is one of a small class of substances of which the action on frogs appears to be identical. As the heart is the organ primarily affected by them, they may be called cardiac poisons, so far as frogs are concerned.

2. These substances are, besides digitaline, the *Upas Antiar*, the *Helleborus viridis*, and perhaps other species of *Helleborus*, the *Tanghinia venenifera*, the *Dajaksch* or arrow-poison of Borneo, the *Carroval* and *Vao*, South American arrow-poisons, and the *Scilla maritima*. Of these we have ourselves experimented only with digitaline, antiar, the *Helleborus viridis* and the *H. niger*, and the *Scilla*; and we believe that we are the first observers who have recognized the identity of the action on frogs of the last of these plants with that of the other substances placed in this group. Besides digitaline, only two of them, namely, the *Helleborus* and the *Scilla*, are likely to be the subject of medico-legal investigation in this country, and that but rarely.

3. The characteristic effect of each of these agents on frogs is the production of irregularity of the heart's action, followed by complete stoppage of its pulsations; the ventricle remaining rigidly contracted, and perfectly pale, after it has ceased to beat; the muscular power of the animal being at this time unimpaired, and persisting as long as in frogs in which the circulation has been stopped by other means, such as ligature of the heart.

The irregularity in the heart's action, which precedes its stoppage, under the influence of these poisons is peculiar. The rhythm is but little altered; and the beats are not necessarily diminished in number, as has been supposed. Sometimes, however, the ventricle makes only one pulsation for two of the auricles, the number of its contractions being therefore lessened

by one half. More frequently the irregularity consists in one or more portions of the ventricle (especially the apex) becoming rigidly white and contracted, while the remainder of the organ continues to dilate regularly. When these yielding pulsations are small, a peculiar appearance, as if the wall of the ventricle formed crimson pouches or protrusions, is produced.

4. No other substance, except those mentioned above, has been found to produce this chain of effects, even in a single experiment. We have ourselves tried nineteen different substances, consisting of vegetable extracts and alkaloids. Of these, *emetina,* and the extract of the *Delphinium staphisagria* caused somewhat similar irregularity of the cardiac beats; but in frogs, poisoned by these agents, the muscular power was always lost before the heart had ceased to beat, and the ventricle stopped in the dilated, and not in the contracted, state.

5. When digitaline is applied endermically to frogs, the characteristic effect is invariably produced, if a sufficient quantity be used. This quantity no doubt varies with the size of the animal, but may be stated generally at $\frac{1}{100}$th of a grain. Quantities less than $\frac{1}{150}$th grain usually produce no effect, or at most only temporary irregularity of the heart's action, of a more or less characteristic kind. The result of the injection of doses larger than $\frac{1}{100}$th grain is to diminish the interval between the administration of the poison and the stoppage of the ventricular beats. This interval appears to be seldom less than six or seven minutes, however large the quantity of digitaline.

6. Very poisonous effects are produced in frogs by the endermic application of alcoholic or acetic extracts of matters vomited by patients, or taken from the human stomach after death. The extracts are less poisonous, if at all, to the higher animals.

7. The symptoms produced by these extracts in frogs are in marked contrast to those caused by the cardiac poisons. Like these agents, the animal extracts impair the action of the heart; but their tendency is to cause paralysis of its muscle, and stoppage in the dilated condition. At the same time, they generally destroy the muscular power of the animal.

8. The cause of the toxic action of these animal extracts has not been ascertained; it is probably not always the same, as the effects produced by different extracts are not perfectly similar. These effects are perhaps the result of the combined action of different substances. They are certainly not caused by bile or pepsine, and probably not by any substance in a state of decay.

9. The vegetable acids, when injected in sufficient quantity, stop the action of the heart more rapidly than any poison with which we are acquainted, the organ remaining distended with blood when it has ceased to beat. The toxic action of the animal extracts is not, however, caused by these acids; for the quantity of them contained in the extracts is too small, and the effect is not diminished by neutralization with an alkali.

10. When digitaline, in quantities of $\frac{3}{4}$–$1\frac{1}{4}$ grain, is added to vomited

matters, or to fluids taken from the human stomach *post mortem*, the extracts obtained from such fluids almost invariably produce on frogs the effects of digitaline.

11. This is due partly to the fact that the action of digitaline is generally more rapid than that of the poisonous constituents of the extracts themselves, but principally to the circumstance that it was necessary to give only small doses of the extracts containing digitaline, in order to get the characteristic action.

12. The method of dialysis fails in many cases to separate digitaline from complex organic mixtures which contain it; and this method is rarely of service in aiding the detection of this poison by the physiological test.

13. When digitaline was administered to dogs in quantities little more than sufficient to destroy life, the extracts derived from the matters vomited by these animals, or from the fluids contained in their stomachs after death (when vomiting was artificially prevented), were found in each of those experiments to produce on frogs unmistakeably the effects characteristic of the presence of one of the cardiac poisons.

NOTE.—Received 18th May, 1865.

We have now to add to the list of "cardiac poisons" the Manganja, an arrow-poison, brought from the Zambesi Expedition by Dr. Kirk. Our attention was directed to this substance, which is the fruit of an Apocynaceous plant, by Dr. Sharpey, who informed us of the results of experiments he had made on its action; and we owe to his kindness the opportunity of confirming his observations by our own experiments.

III. "On the Corrections for Latitude and Temperature in Barometric Hypsometry, with an improved form of Laplace's formula." By ALEXANDER J. ELLIS, F.R.S. Received May 11, 1865.

Adopting the notation in Table I. (p. 284), and the data of M. Mathieu (*Annuaire du Bureau des Longitudes*, 1865, p. 321), Laplace's hypsometrical formula, after some easy transformations, becomes

$$\begin{aligned}h_1-\mathrm{H}_1 &= [\log \mathrm{B}-\log b-\cdot 00007\,.\,(\mathrm{M}'-m')\,.\,]\times[500+\mathrm{A}'+a']\\ &\quad\times\left[\frac{18336}{500\,.\,(1-z\cos 2\,\mathrm{L})}\cdot\left(1+\frac{15926}{6366198}\right)\right]\times\left[1+\frac{h_1+\mathrm{H}_1}{6366198}\right]\\ &=[\log \mathrm{B}-\log b-\cdot 00007\,.\,(\mathrm{M}'-m')]\times[500+\mathrm{A}'+a']\\ &\quad\times\frac{36\cdot 764}{1-z\cos 2\,\mathrm{L}}+\frac{h_1^2-\mathrm{H}_1^2}{\mathrm{R}_1}\quad\ldots\ldots\ldots\ldots\quad(a)\\ &=\mathrm{W}\,.\,\mathrm{T}'\,.\,\mathrm{G}_2+v_1-\mathrm{V}_1\quad\ldots\ldots\ldots\ldots\quad(b)\end{aligned}$$

In the last term in (a), $h_1-\mathrm{H}_1$ represents the product of the three preceding factors, $\mathrm{W}\,.\,\mathrm{T}'\,.\,\mathrm{G}_2$; and z is left for the present undetermined.

If γ be the total increase of gravity in proceeding from the equator to the

pole, the coefficient $z=\gamma\div(2+\gamma)$*, for which most writers employ $\frac{1}{2}\gamma$, as they also commonly use $1+z\cos 2\,L$ for $1\div(1-z\cos 2\,L)$. The values assigned to z by different writers vary considerably. Laplace makes $z=\cdot002837$, and M. Mathieu (*Annuaire*, l. c.) gives $z=\cdot00265$. I have thought it, therefore, advisable first to consult the authorities who have calculated γ directly from pendulum experiments, next to calculate γ from the compression deduced from measurements of arcs†, and then, having determined z for each of these values of γ, to take the mean result to five places of decimals. The pendulum reductions are taken from Baily (*Mem. of Astron. Soc.* 1834, vol. vii. p. 94); the four first reductions are cited on the authority of the *Engl. Cyclop. A. & S.* vol. iv. col. 362, and the fifth from the *Proceedings of the Royal Society*, vol. xiii. p. 270. The following are the results.

Pendulum Experiments.

Baily, final result	$\gamma=\cdot0051449$	$z=\cdot0025659$
Sabine,	$\cdot0051807$	$\cdot0025837$
Airy,	$\cdot0051330$	$\cdot0025599$

Measurements of Arcs.

Airy,	$\gamma=\cdot0053273$	$z=\cdot0026566$
Bessel,	$\cdot0053252$	$\cdot0026555$
Everest,	$\cdot0054530$	$\cdot0027191$
Clarke,	$\cdot0052750$	$\cdot0026306$
Pratt,	$\cdot0052816$	$\cdot0026339$
Mean values	$\gamma=\cdot0052651$	$z=\cdot0026256$

Hence I adopt the value $z=\cdot00263$. This differs from Laplace's value by $\cdot000207$, and from that of M. Mathieu by $\cdot00002$. Viewed in relation to the possible errors which may arise from other sources this correction is slight, but it should be made on the principle advocated by Laplace, that it is assignable (*Méc. Cél.* vol. iv. p. 292). Adopting this value of z and reducing the formula (a) to English feet and Fahrenheit degrees, I have constructed Tables I. and II., which give formulæ and figures for calculating heights with every correction of Laplace, more readily than any other that I have seen. As there is no necessity to interpolate, the Tables are even simpler to use than M. Mathieu's (*Annuaire*, l. c.) or Loomis's (*Astronomy*, p. 390), and they are not only simpler but more complete than Baily's (*Astronomical Tables*, 1827, p. 111), which do not give the cor-

* The term $1-z\cos 2\,L$ represents the ratio of the gravity at latitude L°, to the gravity at latitude 45°, which on the spheroidal theory of the earth's shape is

$$[1+\gamma\,.\,(\sin L)^2]\div(1+\tfrac{1}{2}\gamma),$$

and this gives the above value of z.

† I have used Airy's formula $\gamma=\cdot008668-1\div c$, and not Biot's where the constant is $\cdot00865$, $1\div c$ being the compression.

rection for the variation of gravity on the vertical. They have the further advantage of being applicable to both English and continental measures. The unavoidable uncertainties of the theory make it useless to consider more minute quantities than a foot, or the hundredth of a metre or of a toise. Hence only five-figure logarithms are required. The following examples will show the use of these Tables.

Ex. 1. (*Feet and Fahrenheit.*) Part of Glaisher's Balloon Ascent, 5th Sept. 1862. (*Report of British Association*, 1862.)

B'	20·717	A	32·1	H	9885
b'	17·931	a	25·5	L	53
			836·0		
		T	893·6		

log B'	1·31633	W. T. G	3754
log b'	1·25360	H	9885
		v for 14000	9
W	·06273	V for 10000	−5
log W	8·79748	h	13643
log T	2·95114		
lat. 53°, log G	1·82583		
log (W. T. G)	3·57445		

Ex. 2. (*Metres and Centigrade.*) Mont Blanc, taking St. Bernard as the lower station. (*Ann. Météorol. de France*, 1852.)

B'	·56803	A'	7·6	H_1	2463
b'	·42429	a'	−9·1	L	46
			500·0		
		T'	498·5		

log B'	9·75437	W. T'. G_2	2322
log b'	9·62766	H_1	2463
		v_1 for 4800	3·6
W	·12671	V_2 for 2400	−0·9
log W	9·10281	h_1	4787·7
log T'	2·69767		
lat. 46° }	1·82610		
log G_2 }	9·73928		
log (W. T'. G_2)	3·36586		

Ex. 3. (*Toises and Centigrade.*) Monte Gregorio (cited by Bessel from *D'Aubuisson's Géognosie*, i. 481).

B	329·013	M'	19·85	A'	19·95	H_2	128·3
b	268·215	m'	10·5	a'	9·9	L	46
					500·0		
			9·35				
		×	·00007	T'	529·85		
		t	·00065				

log B	2·51721	log W	8·94488	$W.T'.G_5$	880·2
log b	2·42848	log T'	2·72415	H_2	128·3
		lat. 46°	1·82610	v_2 for 1000	0·3
	·08873	log G_5	9·44946	V_2 for 100	−0·0
t	·00065				
		log $(W.T'.G_5)$	2·94459	h_2	1008·8
W	·08808				

The coefficient 36·764 in (a) results from Ramond's comparison of trigonometrical with barometrical measurements (*Méc. Cél.* iv. 290). Bessel's theory, with the numbers corrected by Plantamour (*Ann. Météor. de F.* 1852), makes it 36·809. If this coefficient were adopted the values of log G in Table II. would have to be increased by ·00053. This would increase the results in the foregoing examples by 4 feet, 2·8 metres, and 1·3 toise respectively. Verification of these numbers by actual levelling is much needed, but it is rendered difficult by the uncertainty attending the correction for temperature*. Thus if $E=1+{\cdot}003665\,.\,\tau$, where τ degrees Centigrade is the temperature of the air at a height of x metres, and $X=R_1x\div(R_1+x)$, it becomes necessary in the determination of the formula to integrate $dX\div E$ (see especially Bessel in *Schumacher's Astron. Nachr.* vol. xv. no. 356. art. 2. eq. 5), and consequently to know the relation between E and X. Laplace then says (*l. c.*), "comme les intégrales ne s'étendent jamais qu'à un intervalle peu considérable, relativement à la hauteur entière de l'atmosphère ; *toute fonction* qui représente à-la-fois les températures des deux stations inférieure et supérieure, et suivant laquelle la température diminue à-peu-près en progression arithmétique de l'une à l'autre, *est admissible,* et l'on peut choisir celle qui simplifie le plus le calcul." Bessel (*l. c.*) says " we are entirely ignorant of this relation, and have therefore no reason to assume the alteration of temperature as otherwise than proportional to the alteration of height." Laplace and Bessel then make an assumption which approximatively fulfils this condition and is equivalent to taking $E^2+k\,.\,X=$ a constant, k being determined by the observed temperatures at the two stations. This makes the integration easy, but it is evident that the result should not be applied in cases where the difference of level is not small in relation to the extent of the appreciable atmosphere, or where the temperature does not diminish approximately as the height increases. Now Mr. Glaisher, as the result of his observations

* The errors in determining the actual temperatures of the air in mountain ascents, arising from the radiation of the ground, are not considered, because they are rather errors of observation than of theory.

on the diminution of temperature with increase of height, gives a series of average decrements such that on assuming the temperature to decrease m degrees Fahrenheit for an elevation of n thousand feet, and representing a degree Fahrenheit and a thousand feet, by a horizontal and a vertical unit of length respectively, we shall find that the resulting curve approaches to a rectangular hyperbola $mn+am+bn=0$, referred to axes parallel to its asymptotes. We may then by the principle of least squares determine the values of a and b from his Tables*. But on comparing such a curve with the curves of alteration of temperature really observed†, the deviation from the average appears so great in particular cases, that no advantage would accrue from complicating the integration by the introduction of such a law.

The only course that appears open to pursue is to confine the limits of the integration to those small amounts which Laplace contemplated in the passage cited, and calculate the height by sections. For it also appears from Mr. Glaisher's curve, that for small alterations of height the alteration of temperature varies approximately as the alteration of height, that is, that the curve does not deviate materially from its tangent for comparatively considerable distances. When the difference of level is many thousand feet the difference of temperature is generally large, and the curve consequently differs materially from a straight line. No dependence can then be placed on the result. It would appear that we should be more likely to obtain correct results by dividing the whole height into a number of partial heights, not exceeding 1000 metres or 3000 feet, and taking fresh observations whenever the temperature altered abnormally. To have a rough notion of when this occurs, an aneroid barometer and common thermometer should be watched on the ascent. Mr. Glaisher's observations tend to show that we may expect on an average a fall of very nearly 4° Fahr. for each inch of depression of the barometer under a *cloudy* sky, the first inch, and the 11th to the 16th inch of depression being accompanied by a slightly more rapid fall of temperature. Under a *clear* or nearly clear sky, there is a fall of about 5° Fahr. for each of the first 4 inches of depression of the barometer; then about 4°·2 per inch from the 5th to the 13th inch, and about 4°·5 per inch from the 14th to the 16th inch‡. This

* In an article in the *Reader* newspaper (31 Oct. 1863, p. 513), purporting to be an extract from Mr. Glaisher's Report to the British Association in 1863 (the passage does not occur in the published Report of the B. A.), it appears, on correcting two obvious misprints, that he has thus calculated $m=5{\cdot}6295\,.\,n\div(1+0{\cdot}048\,.\,n)$, giving $mn+20{\cdot}8333\,.\,m-117{\cdot}281\,.\,n=0$, for which $mn+21m-117n=0$ is a sufficiently close approximation, and represents the mean variation very fairly, after the first 5000 feet of ascent.

† Mr. Glaisher has laid down these in the *Proceedings of the British Meteorological Society*, vol. i. (19 Nov. 1862) plate 13, with which I have compared the theoretical hyperbola.

‡ These comparisons have been obtained by calculating the height attained for each inch of depression of the barometer, from the 1st to the 16th, taking for the bottom

may therefore be considered as the normal alteration of temperature. In order to secure simultaneous observations at both stations for each section, it would be necessary to have two ascending parties, one for each variable station, each of which should be able to signal to the other. A stationary observer at the lowest station would serve as a check on the other two. This method introduces many practical difficulties, but the reduction of the observations is rendered very easy by Tables I. and II. The great importance of thus calculating heights by sections will be rendered evident by the following examples.

Taking the data in the *Ann. Météor. de F.* for 1852, p. 70, we have for Geneva as the lower and St. Bernard as the upper station, L 46,

B'	0·72643	A'	8·97	H_1	408,
b'	0·56364	a'	$-1·89$	h_1	2463.

Again, for St. Bernard as the lower and Mont Blanc as the upper station,

B'	0·56803	A'	7·6	H_1	2463,
b'	0·42429	a'	$-9·1$	h_1	4787·7;

which has been calculated as Ex. 2 above.

But taking the data from the *Annuaire du B. des L.*, 1865, p. 324, we have for Geneva as the lower and Mont Blanc as the upper station,

B	729·65	M'	18·6	A'	19·3	H_1	408,
b	424·05	m'	$-4·2$	a'	$-7·6$	h_1	4815·9.

That is, the height of Mont Blanc above the sea, when calculated from observations at Geneva, St. Bernard, and the summit, is determined as 4787·7 metres, but when calculated from observations at Geneva and the summit only, is determined as 4815·9 metres, or 28·2 metres more. This is striking enough, but it is by no means clear that even the smaller amount may not be too large*.

station B' 30, A 60, H 0, L 45, and supposing the temperature to decrease according to Mr. Glaisher's Tables. The increase of height for each inch of depression was then divided by the number of feet of ascent in which, according to Mr. Glaisher, the temperature falls one degree at the height reached.

* In the *Ann. Mét. de F.* (l. c.) M. Plantamour calculates the height of St. Bernard by Bessel's formula (taking account of the humidity of the atmosphere according to his hypothesis, which is, however, not in accordance with Mr. Glaisher's observations) as 2473 metres. In the *Annuaire de la Société Météorologique de France*, 1853, p. 249, M. Plantamour gives the height of the basin of the barometer at the hospice of St. Bernard as 2493 metres, but does not there state how this result was obtained. These heights being respectively 10 and 30 metres greater than that calculated by Laplace's formula, would, if adopted as the height of the lower station in the second calculation, give results more nearly in accordance with those in the third calculation. The object here, however, is to examine the action of Laplace's formula only, and hence the height assumed for St. Bernard must be that due to that formula. But different data give different results for this height. Geneva and St. Bernard are too widely separated horizontally, and have generally too great a difference of temperature, to enable us to calculate the whole height in one section with any degree of confidence, as there are probably many abnormal intermediate changes of temperature which, as will be seen, tend to vitiate the result. Nor can any reliance be placed on adopting the mean barometric pressures and temperatures. If any mean be taken, it must be the mean of many heights separately calculated from their individual data.

Mr. Glaisher's balloon ascents offer a very convenient series of examples on account of the comparative closeness of his observations. I have therefore calculated two, Tables III. and IV., p. 286, which are important from their height or remarkable changes of temperature, first, by determining the height of each station from the lowest (which I call the *total* method); and secondly, by calculating the height of each station from the height of the next lower station (which I call the *gradual* method). I have added the differences of level between the stations as determined from both methods and the differences between them, which are important for discovering how the discrepancies between the two results are produced by temperature. Each station is lettered. Two letters against a number, as *a h* 5720, show that the height of the station *h* above the sea is found as 5720 feet, when station *a* is taken as the lower station with the height assigned to it in the same column. The distance *a h* is termed an interval. A careful examination of these results will show that the gradual method is probably the most trustworthy.

In Table III. up to station *i*, both results substantially agree, but in the interval *i j* there is a sudden *increase* of temperature, which is quite abnormal*. The total method, from omitting all considerations of the preceding lower temperatures, makes the height of the interval *i j* exceed its value as determined by the gradual method by 59 feet, an enormous amount in a total height of 7518 or 7579 feet. The temperature again decreasing from *j* to *k*, the difference is not so great, but the total method is 8 feet in *defect* for this interval. Again, for *m n* there is only a slight fall of temperature, and consequently the total method, ignoring the low absolute temperature of the interval, makes the difference of level greater than the gradual method by 27 feet. In *p q* there is absolutely a *rise* of temperature, and for the reason last stated, the total method makes the interval 73 feet greater than the gradual. The interval *q r* is a great contrast to this. The temperature falls very rapidly, 7°·1 for a barometric depression of ·79 inch, which is nearly double the normal amount as previously determined for the 14th inch of depression. Hence the total method, by distributing the cold over the warm parts, makes the interval *q r* 73 feet *less* than the gradual method. Again, *r s* shows an excess of 103 feet in the total method for a steady temperature, and *s t* a defect of 100 feet for a sudden fall of temperature. Mr. Glaisher's observations show that there was a rise and fall of temperature between *r* and *s*, but as there were no simultaneous observations of barometer and thermometer, I have not been able to introduce them into the calculation. The results after *r* are therefore very doubtful. The interval *v w* is liable to grave suspicion, not only from the great length of the interval, but the imperfect manner in which the observations were unavoidably made. Supposing the observations to

* It is readily seen that on the assumed law of temperature, $E^2+k \cdot X=$ constant; the sign of $dx \div dt$ depends on that of k, and is therefore supposed to be constant. When therefore $dx \div dt$ alters its sign during part of the height, the law is vitiated, and the formula inapplicable. The only chance of a decent approximation consists in separately calculating the intervals with decreasing and increasing temperatures.

be correct, the total method makes the interval vw greater than the gradual by no less than 610 feet, owing to its distributing the warm temperatures over so large an interval of extreme cold. If we then omit the interval vw, we find 359 feet for the sum of all the cases in which the total method was in excess of the gradual, and 201 feet for the cases of defect, leaving a total excess of 158 feet in 26450 or 26292 feet, which is thus shown to be a very inadequate measure of the degree of uncertainty due to the total method.

In Table IV. the results to c, or even d, substantially agree; but at d the temperature decreases very slowly, and soon becomes absolutely stationary. Great differences immediately appear. From l to r the temperature increases, and the total method gains greatly on the gradual till at r it is 541 feet in advance. At stations s, t the total method indicates a descent with a falling barometer, whereas the gradual method gives a very slow ascent. Mr. Glaisher's observations show that for the same barometric pressure of 14·637 inches, as at r, the temperature varied successively through 36°·1, 38°·2, 38°·1, 42°·2 Fahr., which on the total method indicate different heights, whereas the gradual methods cannot admit any variation of height without a variation of pressure. The rapid fall of the thermometer from u to w causes the total method to give very much smaller intervals than the gradual, but the nearly stationary temperatures of x, y, z turn the balance the other way. On the whole, the total method gives 686 feet in excess, and 335 feet in defect of the gradual method, remaining 351 feet in excess. The temperature varied so abnormally in this ascent that little confidence can be reposed in either result after station h, when the total method is only 32 feet out of 9411 or 9379 in advance of the gradual, which is still a large amount.

It may be objected to the gradual method that, by multiplying stations, it multiplies errors of observation. But even when the stations are so unnecessarily multiplied as in Tables III. and IV. (in which nearly every recorded case of a simultaneous observation of barometer and thermometer has been admitted), the error is not likely to approach that arising from the total method. We may, however, calculate the ascent of Table III. as far as r, beyond which, as already remarked, the variation of temperatures renders the results uncertain, in six instead of sixteen stations, as follows.

Abridged Gradual Method.

Intervals abridged.	Gradual Method.		Difference of Level.		Abridged less Table III.
	Abridged.	Table III.	Abridged.	Table III.	
a	490	490	490	490	0
ad	3655	3655	3165	3165	0
df	5017	5019	1362	1364	− 2
fk	9875	9885	4858	4866	− 8
kn	13633	13640	3758	3755	+ 3
np	17552	17559	3919	3919	0
pr	20339	20357	2787	2798	−11

The final result is 18 feet less than that obtained in Table III. This difference may be easily accounted for. Up to f both results substantially agree. Between f and k there was first a rise and then a fall of temperature, which are overlooked in the abridged calculation, and it consequently loses 8 feet. In the interval $p\ r$ there was a steady temperature during 1400 feet, which disappears in the abridgement, and consequently it again loses 11 feet. It is evident, therefore, that the sections in this abridgement have been badly selected, and the importance of determining them rather by change of temperature than by height ascended becomes apparent. A better result is obtained by means of the seven sections $a\,i$ 6327, $i\,j$ 7520, $j\,k$ 9887, $k\,n$ 13649, $n\,p$ 17568, $p\,q$ 18963, $q\,r$ 20366, determined with reference to the change of temperature. The result, r 20366, is only 9 feet more than that of the gradual method in Table III., but is 104 feet less than that of the total method.

If β, β', β'' be the barometric readings reduced to 32° F., and α, α', α'' the corresponding temperatures of the air for any three stations, then the formula (a) shows that, rejecting the small corrections v_1, V_1, the height, as determined by the total method, will be the same as that determined by the gradual method when

$$(\alpha+\alpha'')\,.\,(\log\beta-\log\beta'')=$$
$$(\alpha+\alpha')\,.\,(\log\beta-\log\beta')+(\alpha'+\alpha'')\,.\,(\log\beta'-\log\beta''),$$

that is, when
$$\frac{\alpha-\alpha'}{\alpha'-\alpha''}=\frac{\log\beta-\log\beta'}{\log\beta'-\log\beta''}.$$

When the difference in barometric pressure is not great, and hence $\beta+\beta'$ is nearly $=\beta'+\beta''$, by applying the reductions in 'Proceedings,' vol. xii. p. 516, the above condition becomes very nearly, that the decrement of temperature should vary as the decrement of pressure, and this is the case for the normal decrements. Thus in Table III. the intervals $a\,i$, $j\,k$, $l\,m$, $n\,p$ give for the quotients of the decrements of temperature divided by the decrements of pressure 4·635, 4·07, 3·26, 3·92 respectively, and the differences of the lengths of these intervals, as determined by the total and gradual methods, are only 2, −8, 13, 13 respectively. But for the intervals $i\,j$, $m\,n$ these quotients are −3·55, 1·27, and the differences 59, 37. Similarly in Table IV., for the intervals $a\,d$, $a\,e$, $a\,h$ the quotients are 4·78, 3·91, 3·97, and the differences −9, 31, 32. These results confirm the above conclusion, and also tend to show that the normal quotient is 4, and to explain why the gradual method is the most generally trustworthy.

Since, then, it is advisable to calculate by such short sections, the practical rules which I gave in a former paper ('Proceedings,' March 26, 1863, vol. xii. pp. 513, 514) may be condensed into one, which will enable any traveller to calculate heights without the assistance of any tables whatever. I conclude this paper, therefore, by annexing it in its improved form, together with a rule calculated on the same principles for foreign data, and an example of each to show the method of working.

Practical Rules without any Tables.

1. *English feet, Fahrenheit temperatures.*

Multiply the difference of the barometric readings in any unit by 52400, and divide by the sum of the barometric readings. [If the result be 1000, 2000, 3000, 4000, or 5000, add 0, 0, 2, 6, 14 respectively.]

Subtract $2\frac{1}{3}$ times the difference of the temperatures of the mercury.

Multiply the remainder by the result of first adding 836 to the sum of the temperatures of the air, next dividing by 900, [and finally

adding for latitude	0,	20,	30,	40,	45,
and subtracting for lat.	90,	70,	60,	50,	45,
the decimals	·0026,	·0020,	·0013,	·0005,	0.]

To this product add the height of the lower station, [and if the sum is

	5000,	10000,	15000,	20000,	25000,
add	1,	5,	11,	19,	30,

subtracting the same numbers when the upper numbers are the heights of the lower station.]

The final result is the height of the upper station above the sea-level according to Laplace's complete formula. [For British heights, the corrections in brackets may be omitted.] Fresh observations should be made whenever the temperature does not decrease about 4 degrees for a fall of one inch in the barometer. Calculate great heights in sections.

Ex. 4. The same data as Ex. 1, with the exception of H being the interval $k\,n$ in the Table of the 'Abridged Gradual Method.'

$$
\begin{array}{rrl}
B' & 20{\cdot}717 & \\
b' & 17{\cdot}931 & \\
\hline
B'+b' & 38{\cdot}648 & \\
\hline
B'-b' & 2{\cdot}786 & \\
\times & 52400 & \\
\hline
\multicolumn{3}{l}{38{\cdot}648)145986{\cdot}400(3777} \\
 & & +6 \\
\hline
 & & 3783 \\
 & & \times{\cdot}9922\,p \\
\hline
\multicolumn{2}{l}{\text{Approximative difference of level} \ldots\ldots} & 3754
\end{array}
\qquad
\begin{array}{rl}
A & 32{\cdot}1 \\
a & 25{\cdot}5 \\
 & 836{\cdot}0 \\
\hline
900) & 893{\cdot}6 \\
\hline
 & {\cdot}9929 \\
 & -{\cdot}0007 \text{ for lat. } 53^\circ \\
\hline
p & {\cdot}9922
\end{array}
\qquad
\begin{array}{rl}
H & 9875 \\
L & 53 \\
 & \\
 & 3754 \\
 & 9875\ H \\
 & +8 \text{ for } 13000 \\
 & -5 \text{ for } 10000 \\
\hline
h & 13632 \text{ feet.}
\end{array}
$$

Since decimals of a foot are rejected, there is always a liability to a difference of 1 or 2 feet between this and the logarithmic method. A difference of 10 feet between this result and that in Ex. 1, is due to the difference in the assumed value of H. [*Continued on page* 288.]

Table I.

NOTATION (CAPITALS, lower station; *small letters,* upper station).

B, b units of length of any kind, height of barometer.

B′, b' the same reduced to 32° Fahr.

A, a deg. Fahr., A′, a' deg. Cent., A″, a'' deg. Réaum., temperature of air.

M, m „ „ M′, m', „ „ M″, m'' „ „ temperature of mercury.

H, h feet, H_1, h_1 metres, H_2, h_2 toises, height above sea.

V, v „ V_1, v_1 „ V_2, v_2 „ correction for height.

R „ R_1 „ R_2 „ mean radius of earth.

$\log R = 7{\cdot}3199534$, $\log R_1 = 6{\cdot}8039605$, $\log R_2 = 6{\cdot}5141407$.

L degrees, mean latitude of the two stations.

$t = {\cdot}000039\,.\,(M-m) = {\cdot}00007\,.\,(M'-m') = {\cdot}000088\,.\,(M''-m'')$.

$T = A + a + 836$; $T' = A' + a' + 500$; $T'' = A'' + a'' + 400$.

$W = \log B - \log b - t = \log B' - \log b'$.

$\log G = \log 60309{\cdot}19 - \log 900 - \log (1 - {\cdot}00263 \cos 2L)$.

$\log G' = \log G + 0{\cdot}25527$	$\log G_3 = \log G + 9{\cdot}83619 - 10$
$\log G'' = \log G + 0{\cdot}35218$	$\log G_4 = \log G + 9{\cdot}19419 - 10$
$\log G_1 = \log G + 9{\cdot}48401 - 10$	$\log G_5 = \log G + 9{\cdot}44946 - 10$
$\log G_2 = \log G + 9{\cdot}73928 - 10$	$\log G_6 = \log G + 9{\cdot}54637 - 10$

1 metre $= 3{\cdot}28090$ feet, 1 toise $= 6{\cdot}39459$ feet, 1 toise $= 1{\cdot}94904$ metres.

$\log 3{\cdot}28090 = {\cdot}51599$, $\log 6{\cdot}39459 = {\cdot}80581$, $\log 1{\cdot}94904 = {\cdot}28982$.

FORMULÆ.

Result.	*Temp.*	
Feet	Fahr.	$h = W.T.\ G + H + v - V$
„	Cent.	$= W.T'.\ G' + H + v - V$
„	Réaum.	$= W.T''.G'' + H + v - V$
Metres	Fahr.	$h_1 = W.T\,.\,G_1 + H_1 + v_1 - V_1$
„	Cent.	$= W.T'.\ G_2 + H_1 + v_1 - V_1$
„	Réaum.	$= W.T''.G_3 + H_1 + v_1 - V_1$
Toises	Fahr.	$h_2 = W.T\,.\,G_4 + H_2 + v_2 - V_2$
„	Cent.	$= W.T'.\ G_5 + H_2 + v_2 - V_2$
„	Réaum.	$= W.T''.G_6 + H_2 + v_2 - V_2$

Log G is found from the latitude in Table II., without interpolation.

V, v; V_1, v_1; V_2, v_2 are found from the nearest number of thousand feet, two hundred metres, or hundred toises in H, h, H_1, h_1, H_2, h_2 respectively, by Table II., without interpolation.

Make fresh observations when the temperature does not decrease about 4° Fahr., or 2° Cent. for a fall in the barometer of 1 inch, or 25 millimetres respectively.

TABLE II.

Lat.	Log G.	Lat.	Log G.	Feet.		Metres.		Toises.	
				h, H ÷1000.	v, V.	h_1, H_1 ÷100.	v_1, H_1.	h_2, H_2 ÷100.	v_2, V_2.
0°	1·827 28	45°	1·826 14	0	0	0	0·0	0	0·0
1	28	46	10	1	0	2	0·0	1	0·0
2	28	47	06	2	0	4	0·0	2	0·0
3	28	48	1·826 02	3	0	6	0·1	3	0·0
4	27	49	1·825 98	4	1	8	0·1	4	0·1
5	1·827 27	50	94	5	1	10	0·2	5	0·1
6	26	51	90	6	2	12	0·2	6	0·1
7	25	52	86	7	2	14	0·3	7	0·2
8	24	53	83	8	3	16	0·4	8	0·2
9	23	54	79	9	4	18	0·5	9	0·3
10	1·827 22	55	1·825 75	10	5	20	0·6	10	0·3
11	20	56	71	11	6	22	0·8	11	0·4
12	19	57	68	12	7	24	0·9	12	0·4
13	17	58	64	13	8	26	1·1	13	0·5
14	15	59	61	14	9	28	1·2	14	0·6
15	1·827 13	60	1·825 57	15	11	30	1·4	15	0·7
16	11	61	54	16	12	32	1·6	16	0·8
17	09	62	50	17	14	34	1·8	17	0·9
18	07	63	47	18	16	36	2·0	18	1·0
19	04	64	44	19	17	38	2·3	19	1·1
20	1·827 02	65	1·825 41	20	19	40	2·5	20	1·2
21	1·826 99	66	38	21	21	42	2·8	21	1·4
22	96	67	35	22	23	44	3·0	22	1·5
23	94	68	32	23	25	46	3·3	23	1·6
24	91	69	29	24	28	48	3·6	24	1·8
25	1·826 88	70	1·825 27	25	30	50	3·9	25	1·9
26	84	71	24	26	32	52	4·3	26	2·0
27	81	72	22	27	35	54	4·6	27	2·2
28	78	73	20	28	38	56	4·9	28	2·4
29	75	74	17	29	40	58	5·3	29	2·6
30	1·826 71	75	1·825 15	30	43	60	5·7	30	2·8
31	68	76	13	31	46	62	6·0	31	2·9
32	64	77	12	32	49	64	6·4	32	3·1
33	61	78	10	33	52	66	6·8	33	3·3
34	57	79	08	34	55	68	7·3	34	3·5
35	1·826 53	80	1·825 07	35	59	70	7·7	35	3·8
36	49	81	06	36	62	72	8·1	36	4·0
37	46	82	04	37	65	74	8·6	37	4·2
38	42	83	03	38	69	76	9·1	38	4·4
39	38	84	03	39	73	78	9·6	39	4·7
40	1·826 34	85	1·825 02	40	77	80	10·1	40	4·9
41	30	86	01	41	80	82	10·6	41	5·2
42	26	87	01	42	84	84	11·1	42	5·4
43	22	88	00	43	89	86	11·6	43	5·7
44	18	89	00	44	93	88	12·2	44	5·9
45	1·826 14	90	1·825 00	45	97	90	12·7	45	6·2

TABLE III.—Mr. Glaisher's Ascent from Wolverhampton, lat. 53°, 5 September, 1862.

Station.	B′.	A.	Interval.	Total method.	Interval.	Gradual method.	Difference of Level.		Total less Gradual.
							Total method.	Gradual method.	
a	29·40	59·5	*a*	490	*a*	490	490	490	0
b	29·17	59·0	*ab*	708	*ab*	708	218	218	0
c	28·38	55·5	*ac*	1467	*bc*	1467	759	759	0
d	26·19	45·5	*ad*	3655	*cd*	3655	2188	2188	0
e	24·994	42·0	*ae*	4917	*de*	4911	1262	1256	6
f	24·894	40·7	*af*	5021	*ef*	5019	104	108	− 4
g	24·30	39·5	*ag*	5671	*fg*	5663	650	644	6
h	24·25	38·0	*ah*	5720	*gh*	5718	49	55	− 6
i	23·70	36·5	*ai*	6327	*hi*	6325	607	607	0
j	22·658	40·0	*aj*	7579	*ij*	7518	1252	1193	59
k	20·717	32·1	*ak*	9938	*jk*	9885	2359	2367	− 8
l	20·17	31·2	*al*	10646	*kl*	10586	708	701	7
m	18·727	26·5	*am*	12590	*lm*	12517	1944	1931	13
n	17·931	25·5	*an*	13740	*mn*	13640	1150	1123	27
p	15·38	15·5	*ap*	17672	*np*	17559	3932	3919	13
q	14·553	15·6	*aq*	19140	*pq*	18954	1468	1395	73
r	13·76	8·5	*ar*	20470	*qr*	20357	1330	1403	− 73
s	12·754	8·1	*as*	22459	*rs*	22243	1989	1886	103
t	11·954	0·0	*at*	23955	*st*	23839	1496	1596	−100
u	11·254	− 2·0	*au*	25472	*tu*	25304	1517	1465	52
v	10·803	− 5·0	*av*	26450	*uv*	26292	978	988	− 10
w	7·0	−12·0	*aw*	37427	*vw*	36659	10977	10367	610

TABLE IV.—Mr. Glaisher's Ascent from Wolverhampton, lat. 53°, 17 July, 1862.

Station.	B′.	A.	Interval.	Total Method.	Interval.	Gradual Method.	Difference of Level.		Total less Gradual.
							Total Method.	Gradual Method.	
a	29·193	59·0	*a*	490	*a*	490	490	490	0
b	26·014	45·0	*a b*	3643	*a b*	3643	3153	3153	0
c	25·215	43·0	*a c*	4487	*b c*	4481	844	838	6
d	24·138	34·8	*a d*	5633	*c d*	5642	1146	1161	− 15
e	22·421	32·5	*a e*	7612	*d e*	7581	1979	1939	40
f	22·023	32·5	*a f*	8095	*e f*	8050	483	469	14
g	21·575	29·8	*a g*	8620	*f g*	8588	525	538	− 13
h	20·927	26·2	*a h*	9411	*g h*	9379	791	791	0
i	19·629	26·0	*a i*	11127	*h i*	11035	1716	1656	60
j	19·281	26·0	*a j*	11633	*i j*	11497	506	462	44
k	18·633	26·0	*a k*	12524	*j k*	12381	891	884	7
l	18·136	24·9	*a l*	13233	*k l*	13079	709	698	11
m	17·235	31·0	*a m*	14693	*l m*	14402	1460	1323	137
n	16·735	31·6	*a n*	15495	*m n*	15173	802	771	31
p	16·036	32·0	*a p*	16655	*n p*	16290	1160	1117	43
q	14·937	37·2	*a q*	18673	*p q*	18163	2018	1873	145
r	14·637	38·2	*a r*	19243	*q r*	18702	570	539	31
s	14·634	36·5	*a s*	19214	*r s*	18707	− 29	5	− 34
t	14·633	34·0	*a t*	19164	*s t*	18709	− 50	2	− 52
u	14·134	31·5	*a u*	20052	*t u*	19620	888	911	− 23
v	13·637	24·5	*a v*	20864	*u v*	20551	812	931	− 119
w	13·137	19·2	*a w*	21742	*v w*	21508	878	957	− 79
x	12·139	17·5	*a x*	23804	*w x*	23519	2062	2011	51
y	11·741	16·0	*a y*	24654	*x y*	24365	850	846	4
z	11·143	16·0	*a z*	26039	*y z*	25688	1385	1323	62

2. *French metres, Centigrade temperatures.*

Multiply the difference of the barometric readings in any unit by 16000, and divide by the sum of the barometric readings. If the result be 300, 600, 900, 1200, subtract 0·6, 0·9, 0·9, 0·2; if 1300, 1600, add 0·2, 2·0 respectively.

Subtract 1·3 times the difference of the temperatures of the mercury.

Multiply the remainder by the result of first adding 500 to the sum of the temperatures of the air, then dividing by 500, and finally

adding for latitude	0,	20,	30,	40,	45,
and subtracting for lat.	90,	70,	60,	50,	45,
the decimals	·0026,	·0020,	·0013,	·0005,	0.

To this product add the height of the lower station; and if the sum is

	1000,	2000,	3000,	4000,	5000,	6000,	7000,	8000
add	·2,	·6,	1·4,	2·5,	3·9,	5·7,	7·7,	10·1,

subtracting the same numbers when the upper numbers are the height of the lower station.

Fresh observations should be taken whenever the temperature does not decrease about 2° for a fall of 25 millimetres in the barometer. Calculate great heights in sections.

Ex. 5. Height of St. Cergues, in the Canton de Vaud, on the road from Paris to Geneva, lat. 46°. (*Ann. Météor. de Fr.*, 1849, p. 59.)

B 729·71	M′ 21·5	A′ 21·8	H_1 408
b 676·73	*m*′ 18·8	*a*′ 18·8	
		500·0	
B+*b* 1406·44	2·7		
	× 1·3	500)540·6	
B−*b* 52·98			
× 16000	*q* 3·5	1·0812	
		−·0001 lat. 46°	
1406·44)847680·00(602·7			
−·9		*p* 1·0811	
			598·3
601·8			× 1·0811 *p*
−3·5 *q*			
			646·8
app. diff. of level 598·3			408·0 H_1
			+ ·2 for 1000
			h_1 1055 metres.

IV. "On the Elasticity and Viscosity of Metals." By Prof. W. Thomson, LL.D., F.R.S., F.R.S.E. Received May 18, 1865.

Among the experimental exercises performed by students in the physical laboratory of the University of Glasgow, observations on the elasticity of metals have been continued during many years. Numerous questions of great interest, requiring more thorough and accurate investigation, have been suggested by these observations; and recently they have brought to light some very unexpected properties of metallic wires. The results stated in the present communication are, however, with one or two exceptions, due to the careful experimenting of Mr. Donald Macfarlane, official assistant to the Professor of Natural Philosophy, whose interested and skilful cooperation have been most valuable in almost everything I have been able to attempt in the way of experimental investigation.

The subject has naturally fallen into two divisions, Viscosity, and Moduli of Elasticity.

Viscosity.—By induction from a great variety of observed phenomena, we are compelled to conclude that no change of volume or of shape can be produced in any kind of matter without dissipation of energy. Even in dealing with the *absolutely perfect* elasticity of volume presented by every fluid, and possibly by some solids, as for instance homogeneous crystals, dissipation of energy is an inevitable result of every change of volume, because of the accompanying change of temperature, and consequent dissipation of heat by conduction or radiation. The same cause gives rise necessarily to some degree of dissipation in connexion with every change of shape of an elastic solid. But estimates founded on the thermodynamic theory of elastic solids, which I have given elsewhere *, have sufficed to prove that the loss of energy due to this cause is small in comparison with the whole loss of energy which I have observed in many cases of vibration. I have also found, by vibrating a spring alternately in air of ordinary pressure, and in the exhausted receiver of an air-pump, that there is an internal resistance to its motions immensely greater than the resistance of the air. The same conclusion is to be drawn from the observation made by Kupffer in his great work on the elasticity of metals, that his vibrating springs subsided much more rapidly in their vibrations than rigid pendulums supported on knife-edges. The subsidence of vibrations is probably more rapid in glass than in some of the most elastic metals, as copper, iron, silver, aluminium †; but it is much more rapid than in glass, marvellously rapid indeed, in some metals (as for instance zinc) ‡, and in india rubber, and even in homogeneous jellies.

* "On the Thermo-elastic Properties of Solids," Quarterly Journal of Mathematics, April 1857.

† We have no evidence that the precious metals are more elastic than copper, iron, or brass. One of the new bronze pennies gives quite as clear a ring as a two-shilling silver piece tested in the usual manner.

‡ Torsional vibrations of a weight hung on a zinc wire subside so rapidly, that it

The *frictional resistance* against change of shape must in every solid be infinitely small when the change of shape is made at an infinitely slow rate, since, if it were finite for an infinitely slow change of shape, there would be infinite rigidity, which we may be sure does not exist in nature*. Hence there is in elastic solids a *molecular friction* which may be properly called *viscosity of solids*, because, as being an internal resistance to change of shape depending on the rapidity of the change, it must be classed with fluid molecular friction, which by general consent is called *viscosity of fluids*. But, at the same time, it ought to be remarked that the word viscosity, as used hitherto by the best writers, when solids or heterogeneous semisolid-semifluid masses are referred to, has not been distinctly applied to molecular friction, especially not to the molecular friction of a highly elastic solid within its limits of high elasticity, but has rather been employed to designate a property of slow continual yielding through very great, or altogether unlimited, extent of change of shape, under the action of continued stress. It is in this sense that Forbes, for instance, has used the word in stating that "Viscous Theory of Glacial Motion" which he demonstrated by his grand observations on glaciers. As, however, he, and many other writers after him, have used the words plasticity and plastic, both with reference to homogeneous solids (such as wax or pitch even though also brittle, soft metals, &c.), and to heterogeneous semisolid-semifluid masses (as mud, moist earth, mortar, glacial ice, &c.), to designate the property † common to all those cases of experiencing, under continued stress, either quite continued and unlimited change of shape, or gradually very great change at a diminishing (asymptotic) rate through infinite time, and as the use of the term *plasticity* implies no more than does *viscosity* any physical theory or explanation of the property, the word viscosity is without inconvenience left available for the definition I propose.

To investigate the viscosity of metals, I have in the first place taken them in the form of round wires, and have chosen torsional vibrations, after the manner of Coulomb, for observation, as being much the easiest way to arrive at definite results. In every case one end of the wire was attached to a rigid vibrator with sufficient firmness (thorough and smooth soldering I find to be always the best plan when the wire is thick enough);

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has been found scarcely possible to count more than twenty of them in one case experimented on.

* Those who believe in the existence of indivisible, infinitely strong and infinitely rigid very small bodies (finite atoms!) may deny this.

† Some confusion of ideas on the part of writers who have professedly objected to Forbes's theory while really objecting only (and I believe groundlessly) to his usage of the word viscosity, might have been avoided if they had paused to consider that no one physical explanation can hold for those several cases, and that Forbes's theory is merely the proof by observation that glaciers have the property that mud (heterogeneous), mortar (heterogeneous), pitch (homogeneous), water (homogeneous), all have of changing shape indefinitely and continuously under the action of continued stress.

and the other to a fixed rigid body, from which the wire hangs, bearing the vibrator at its lower end. I arranged sets of observations to be made for the separate comparisons of the following classes:—

(*a*) The same wire with different vibrators of equal weights (to give equal stretching-tractions), but different moments of inertia (to test the relation between viscous resistances against motions with different velocities through the same range and under the same stress).

(*b*) The same wire with different vibrators of equal moments of inertia but unequal weights (to test the effect of different longitudinal tractions on the viscous resistance to torsion under circumstances similar in all other respects).

(*c*) The same wire and the same vibrator, but different initial ranges in successive experiments (to test an effect unexpectedly discovered, by which the subsidence of vibrations from any amplitude takes place at very different rates according to the immediately previous molecular condition, whether of quiescence or of recurring change of shape through a wider range).

(*d*) Two equal and similar wires, with equal and similar vibrators, one of them kept as continually as possible in a state of vibration, from day to day; the other kept at rest, except when vibrated in an experiment once a day (to test the effect of continued vibration on the viscosity of a metal).

Results.

(*a*) It was found that the loss of energy in a vibration through one range was greater the greater the velocity (within the limits of the experiments); but the difference between the losses at low and high speeds was *much less* than it would have been had the resistance been, as Stokes has proved it to be in fluid friction, approximately as the rapidity of the change of shape. The irregularities in the results of the experiments which up to this time I have made, seem to prove that much smaller vibrations (producing less absolute amounts of distortion in the parts of the wires most stressed) must be observed before any simple law of relation between molecular friction and velocity can be discovered.

(*b*) When the weight was increased, the viscosity was always at first much increased; but then day after day it gradually diminished and became as small in amount as it had been with the lighter weight. It has not yet been practicable to continue the experiments long enough in any case to find the limit to this variation.

(*c*) The vibration subsided in aluminium wires much more rapidly from amplitude 20 to amplitude 10, when the initial amplitude was 40, than when it was 20. Thus, with a certain aluminium wire, and vibrator No. 1 (time of vibration one way 1·757 second), in three trials the numbers of vibrations counted were—

	Vibrations.	Vibrations.	Vibrations.
Subsidence from 40 initial amplitude to 20	56	64	64
And from 20 (in course of the same experiments) to 10 ..	96	98	96

The same wire and same vibrator showed—

Subsidence from 20 initial amplitude to 10 (average of four trials)............	112 vibrations.

Again the same wire with vibrator No. 2* (time of vibration one way 1·236), showed in two trials—

	Vibrations.	Vibrations.
Subsidence from 40 initial amplitude to 20	54	52
And continued from 20 to 10......	90	90

Again same wire and vibrator,—

From initial amplitude 20 to 10.. 103 vibrations (mean of eight trials).

This remarkable result suggested the question (*d*).

(*d*) Only one comparison was made. It showed in a wire which was kept vibrating nearly all day, from day to day, after several days, very much more molecular friction than in another kept quiescent except during each experiment. Thus two equal and similar pieces of wire were put up about the 26th of April, hanging with equal and similar lead weights, the tops and bottoms of the two wires being similarly fixed by soldering. No. 2 was more frequently vibrated than No. 1 for a few days at first, but no comparison of viscosities was made till May 15. Then

No. 1 subsided from 20 initial range to 10 in 97 vibrations.

No. 2, the same subsidence in 77 vibrations.

During the greater part of May 16 and 17, No. 2 was kept vibrating, and No. 1 quiescent, and late on May 17 experiments with the following results were made:—

					Time per vibration.
No. 1.	Subsided from 20 to 10 after	99 vibrations	in 237 seconds	......	2·4
,,	,,	98 ,,	235 ,,	......	2·4
,,	,,	98 ,,	235 ,,	......	2·4
No. 2.	Subsided from 20 to 10 after	58 ,,	142 ,,	......	2·45
,,	,,	60 ,,	147 ,,	......	2·45
,,	,,	57 ,,	139 ,,	......	2·45
,,	,,	60 ,,	147 ,,	......	2·45

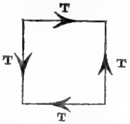
[Addition, May 27, since the reading of the paper.]—No. 1 has been kept at rest from May 17, while No. 2 has been kept oscillating more or less every day, till yesterday, May 26, when both were oscillated, with the following results:—

			Time per vibration.
No. 1.	Subsided from 20 to 10 after	100 vibrations in 242 seconds	2·42
,, 2.	,, ,,	44 or 45 vibrations	2·495

Moduli of Elasticity.—A *modulus* of elasticity is the number by which the amount of any specified stress, or component of a stress, must be divided to find the strain, or any stated component of the strain, which it produces. Thus the cubic compressibility of water being $\frac{1}{21000}$ per atmosphere, its "modulus of compressibility" or its "volume modulus of elasticity," is 21000 atmospheres, or $76 \times 13{\cdot}596 \times 21000 = 21{\cdot}7 \times 10^6$ grammes weight

* Of same weight as No. 1, but different moment of inertia.

per square centimetre (as 13·596 is the density or specific gravity* of mercury, and 76 centimetres the height of the barometric column corresponding to the pressure defined as "one atmosphere"). Or, again, Young's "modulus," which has generally been called simply *the* modulus of elasticity of a solid, is the longitudinal traction of a stretched rod or wire of the substance, divided by the extension produced by it. Or, lastly, the "modulus of rigidity," or, as it is conveniently called, simply "the rigidity" of an isotropic solid, is the amount of tangential stress divided by the deformation it produces,—the former being measured in units of force per unit of area applied, as shown in the diagram, to each of four faces of a cube, and the latter by the variation of each of the four right angles, reckoned in circular measure.



Measurements of Young's modulus have been made for many bodies by many experimenters; but hitherto there have been very few determinations of rigidity, notwithstanding the great ease with which this can be done for wires by Coulomb's method. Accordingly, although several accurate determinations of Young's modulus have been made upon wires of different substances hung in the College Tower of the University of Glasgow (which, by giving 80 feet of clear protected vertical space, affords great facilities for the investigation), I shall in this paper only refer to some of the results as bearing on the question, *how are moduli of elasticity affected in one substance by permanent changes in its molecular condition*? which was my starting-point for all I have attempted to do experimentally regarding the elasticity of solids.

To determine rigidities by torsional vibrations, taking advantage of an obvious but most valuable suggestion made to me by Dr. Joule, I used as vibrator in each case a thin cylinder of sheet brass, turned true outside and

* The one great advantage of the French metrical system is, that the mass of the unit volume (1 centimetre) of water at its temperature of maximnm density (3°·945 Cent.) is unity (1 gramme) to a sufficient degree of approximation for almost all practical purposes. Thus, according to this system, the density of a body and its specific gravity mean one and the same thing; whereas on the British no-system the density is expressed by a number found by multiplying the specific gravity by one number or another, according to the choice (of a cubic inch, cubic foot, cubic yard, or cubic mile) that is made for the unit of volume, and the weight of a grain, scruple, gun-maker's drachm, apothecary's drachm, ounce Troy, ounce avoirdupois, pound Troy, pound avoirdupois, stone (Imperial, Ayrshire, Lanarkshire, Dumbartonshire), stone for hay, stone for corn, quarter (of a hundredweight), quarter (of corn), hundredweight, or ton, that is chosen for unit of force. It is a remarkable phenomenon, belonging rather to moral and social than to physical science, that a people tending naturally to be regulated by common sense should voluntarily condemn themselves, as the British have so long done, to unnecessary hard labour in every action of common business or scientific work related to measurement, from which all the other nations of Europe have emancipated themselves. I have been informed, through the kindness of Professor W. H. Miller, of Cambridge, that he concludes, from a very trustworthy comparison of standards by Kupffer, of St. Petersburgh, that the weight of a cubic decimetre of water at temperature of maximum density is 1000·013 grammes.

inside (of which the radius of gyration must be, to a very close degree of approximation, the arithmetic mean of the radii of the outer and inner cylindrical surfaces), supported by a thin flat rectangular bar, of which the square of the radius of gyration is one-third of the square of the distance from the centre to the corners. The wire to be tested passed perpendicularly through a hole in the middle of the bar, and was there firmly soldered. The cylinder was tied to the horizontal bar by light silk threads, so as to hang with its axis vertical.

The following particulars show the dimensions of the vibrators of this kind which I have used.

Cylinders.	Outer diameter.	Inner diameter.	Mean radius.	Weight in grammes.	Moment of inertia round axis in gramme-centimetres.
No. 1	15·3 centims.	14·8 centims.	7·525	527·92	29894
„ 2	15·3 „	14·8 „	7·525	523·45	29641
„ 3	10·295 „	9·79 „	5·021	360·54	9089
„ 4	10·3 „	9·81 „	5·027	726·40	18357
„ 5	10·25 „	9·745 „	4·999	718·36	17952
„ 6	10·295 „	9·805 „	5·025	342·45	8647

	Length.	Breadth.	Weight.	Moment of inertia round axis through middle, perpendicular to length and breadth.
Bar 1	24·03 centims.	·965 centim.	38·955 grms.	1877·5
„ 2	24·11 „	·95 „	46·68 „	2255·5

Towards carrying out the chief object of the investigation, each wire, after having been suspended and stretched with just force enough to make it as nearly straight as was necessary for accuracy, was vibrated. Then it was stretched by hand (applied to the cross bar soldered to its lower end) and vibrated again, stretched again and vibrated again, and so till it broke. The results, as shown in the following Table, were most surprising.

Length of wire, in centimetres, l.	Volume, in cubic centimetres, V.	Density.	Moment of inertia of vibrator, Wk^2.	Time of vibration one way (or half-period), in seconds, T.	Rigidity, in grammes weight per square centimetre, $\frac{2\pi^3 l^3 Wk^2}{gT^2V^2}$.	Substances.
60·3	1·1845	2·764	31771	1·14	241×10^6	Aluminium [a].
304·9	2·351	7·105	31896	4·31	$359·6\times10^6$	Zinc [b].
237·7	......	......	......	4·76	$410·3\times10^6$	Brass.
248·3	......	......	......	5·456	$354·8\times10^6$	„

Remarks.

[a] Only forty vibrations from initial arc of convenient amplitude could be counted. Had been stretched considerably before this experiment.

[b] So viscous that only twenty vibrations could be counted. Broke in stretching.

TABLE (*continued*).

Length of wire, in centimetres, l.	Volume, in cubic centimetres, V.	Density.	Moment inertia of vibrator, Wk^2.	Time of vibration one way (or half-period), in seconds, T.	Rigidity, in grammes weight per square centimetre, $\frac{2\pi^3 l^3 W k^2}{g\ T^2 V^2}$.	Substances.
261·9	1·703	8·398	......	5·96	$350·1\times10^6$	Brass.
2435·0	15·30	8·91	38186	16·375	$448·7\times10^6$	Copper.
,,	,,	,,	61412	20·77	$448·4\times10^6$	,,
214·4	1·348	8·864	31771	5·015	$433·0\times10^6$	Copper [c].
,,	,,	,,	61412	6·982	$431·8\times10^6$	,,
143·7	·9096	8·674	......	3·381	$393·4\times10^6$	Copper [d].
286·8	......	......	20612	4·245	$442·9\times10^6$	Copper [e].
291	......	......	,,	4·375	$435·6\times10^6$	,,
293	......	......	,,	4·417	$436·2\times10^6$	,,
296·1	......	......	,,	4·500	$433·8\times10^6$	,,
300·0	......	......	,,	4·588	$434·0\times10^6$	,,
303·4	......	......	,,	4·646	$437·8\times10^6$	,,
309·3	......	......	,,	4·833	$428·6\times10^6$	,,
313·2	......	......	,,	4·931	$427·5\times10^6$	,,
317·4	1·962	8·835	,,	5·040	$425·9\times10^6$	,,
215·6	......	......	31771	8·155	$442·3\times10^6$	Copper [f].
235·5	......	......	,,	9·425	$432·2\times10^6$	,,
251·9	·827	8·872	,,	10·463	$428·6\times10^6$	,,
253·2	1·580	8·91	......	5·285	$472·9\times10^6$	Copper [g].
262·8	......	......	......	5·640	$464·3\times10^6$	,,
270·4	......	......	......	5·910	$460·4\times10^6$	,,
278·7	......	......	......	6·20	$458·5\times10^6$	,,
287·9	......	......	......	6·5325	$455·0\times10^6$	,,
297·5	......	......	......	6·8195	$451·0\times10^6$	,,
308·8	......	......	......	7·3075	$448·9\times10^6$	,,
256·5	1·6145	8·90	......	4·226	$463·5\times10^6$	Copper [h].
267·9	......	......	......	4·5625	$453·3\times10^5$	,,
280·1	......	......	......	4·915	$446·2\times10^6$	,,
292·2	......	......	......	5·240	$445·5\times10^6$	,,
301·9	......	......	......	5·532	$438·2\times10^6$	,,
316·8	......	......	......	6·655	$791·4\times10^6$	Soft iron [i].
322·1	......	......	......	6·88	$778·3\times10^6$	,,
335·1	......	......	......	7·301	$779·0\times10^6$	,,
347·4	......	......	......	7·768	$766·6\times10^6$	,,
366·0	1·357	7·657	......	8·455	$756·0\times10^6$	,,
39·4	·1745	20·805	20612	2·05	$622·25\times10^6$	Platinum [k].
65·9	·1825	19·8	10902	......	$281\ \times10^6$	Gold [l].
75·7	·1185	10·21	10967	......	$270\ \times10^6$	Silver [l].

Remarks.

[c] A piece of the preceding stretched.

[d] The preceding made red-hot in a crucible filled with powdered charcoal and allowed to cool slowly, became very brittle: a part of it with difficulty saved for the experiment.

[e] Another piece of the long (2435 centims.) wire; stretched by successive simple tractions.

[f] A finer-gauge copper wire; stretched by successive tractions.

[g] Old copper wire, softened by being heated to redness and plunged in water. A length of 260 centims. cut from this, suspended, and elongated by successive tractions.

[h] Another length of 260 centims. cut from the same and similarly treated.

[i] One piece, successively elongated by simple tractions till it broke.

[k] Not stretched yet for a second experiment.

[l] Added, May 27, after the reading of the paper.

Thus it appears that that specific rigidity which is concerned in torsion is very markedly diminished in copper, brass, and iron wire when the wire is elongated permanently by a simple longitudinal traction. When I first observed indications of this result, I suspected that the diminution in the torsional rigidity on the whole length of the wire might be due to inequalities in its normal section produced by the stretching. To test this, I cut the wire into several pieces after each series of experiments, and weighed the pieces separately. The result proved that in no case were there any such inequalities in the gauge of the wire in different parts as could possibly account for the diminution in the torsional rigidity of the whole, which was thus proved to be due to a real diminution in the specific rigidity of the substance. The following sets of weighings, for the cases of the wires of the two last series of experiments on copper, may suffice for example :—

Wire of 308·8 centims. long, cut into four pieces.

	Length, in centimetres.	Weight, in grammes.	Weight per centimetre, in grammes.
No. 1	109·2	5·023	·04600
„ 2	66·7	3·050	·04573
„ 3	63·2	2·865	·04533
„ 4	69·4	3·143	·04517
	308·5	14·081	

Wire of 301·9 when last vibrated; further elongated by about 8 centimetres, when it broke; then cut into five pieces in all.

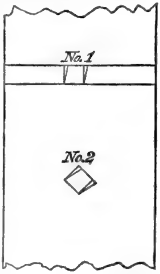
	Length, in centimetres.	Weight, in grammes.	Weight per centimetre, in grammes.
No. 1	66·3	3·183	·04801
„ 2	66·4	3·083	·04643
„ 3	66·5	3·039	·04570
„ 4	66·8	3·072	·04599
„ 5	43·4	1·986	·04576

By several determinations of observations on the elongations within the limits of elasticity produced by hanging weights on long wires (about 80 feet) suspended in the College tower, it seemed that Young's modulus was not nearly so much (if at all sensibly) altered by the change of molecular condition so largely affecting the rigidity; but this question requires further investigation. The amount of the Young's modulus thus found was, in grammes weight per square centimetre, 1159×10^6 for one copper wire, and 1153×10^6 for another which had been very differently treated.

The highest and lowest rigidities which I have found for copper (extracted from the preceding Table) are as follows:—

Highest rigidity, 473×10^6, being that of a wire which had been softened by heating it to redness and plunging it into water, and which was found to be of density 8·91. Lowest rigidity $393{\cdot}4 \times 10^6$, being that of a wire which had been rendered so brittle by heating it to redness surrounded by powdered charcoal in a crucible and letting it cool very slowly, that it could scarcely be touched without breaking it, and which had been found to be reduced in density by this process to as low as 8·674. The wires used were all commercial specimens—those of copper being all, or nearly all, cut from hanks supplied by the Gutta Percha Company, having been selected as of high electric conductivity, and of good mechanical quality, for submarine cables.

It ought to be remarked that the change of molecular condition produced by permanently stretching a wire or solid cylinder of metal is certainly a change from a condition which, if originally isotropic, becomes æolotropic* as to some qualities†, and that the changed conditions may therefore be presumed to be æolotropic as to elasticity. If so, the rigidities corresponding to the direct and diagonal distortions (indicated by No. 1 and No. 2 in the sketch) must in all probability become different from one another when a wire is permanently stretched, instead of being equal as they must be when its substance is isotropic. It becomes, therefore, a question of extreme interest to find whether rigidity No. 2 is not *increased* by this process, which, as is proved by the experiments above described, diminishes, to a very remarkable degree, the rigidity No. 1. The most obvious experiment, and indeed the only practicable experiment, adapted to answer this question, will require an accurate determination of the difference produced in the *volume* of a wire by applying and removing longitudinal traction within its limits of elasticity. With the requisite apparatus a most important and interesting investigation might thus be made.



V. "On Two New Forms of Heliotrope." By W. H. Miller, M.A., For. Sec. R.S., and Professor of Mineralogy in the University of Cambridge. Received May 17, 1865.

A heliotrope is a mirror O provided with some contrivance for adjusting it so that any given distant point T may receive the light of the sun S

* A term introduced to designate a substance which has varieties of property in various directions (Thomson and Tait's 'Natural Philosophy,' § 676).

† See, for example, a paper by the author, "On Electrodynamic Qualities of Metals," Philosophical Transactions, 1856.

reflected from the surface of the mirror. This instrument has been constructed on three different principles. In Drummond's (Philosophical Transactions for 1826, p. 324), by a simple mechanism, a normal to the mirror is made to bisect the angle between the axes of two telescopes, one of which is pointed to T, and the other to S; consequently T will receive the light of S reflected from O. In Struve's (Breitengradmessung, p. 49) the mirror is directed by means of two sights attached to its support, which are brought into the line OT. The heliotrope employed in the Ordnance Survey (Ordnance Trigonometrical Survey of Great Britain and Ireland, Account of Observations and Calculations of the Principal Triangles, p. 47) is similar to Struve's, except that a single mark placed at a convenient distance in the line OT is substituted for the two sights. In the two heliotropes invented by Gauss (Astronomische Nachrichten, vol. v. p. 329, and v. Zach's Correspondance Astronomique, vol. v. p. 374, and vol. vi. p. 65), in Steinheil's (Schumacher's Jahrbuch für 1844, p. 12), and in Galton's an optical contrivance is connected with the mirror, so as to throw a cone of sunlight in a direction opposite to the cone of sunlight reflected from the surface of the mirror, the axes of the two cones being parallel, and either very nearly or absolutely coincident. Hence any point T, from which a portion of the former cone of light appears to proceed, will receive the light of the sun reflected from the mirror.

The heliotropes I am about to describe produce two cones of sunlight thrown in opposite directions, like those of Gauss, Steinheil, and Galton, but differ from them in having no moveable parts, and from all but Galton's, and the sextant-heliotrope of Gauss, with a second moveable mirror, in requiring no support except the hand of the operator.

One of these consists of a plane mirror, to an edge of which are attached two very small plane reflectors, *a*, *c*, forming with one another a reentrant angle of 90°, and making angles of 90° with the faces of the mirror. If a ray be reflected once by each of the two planes *a*, *c*, it is obvious that the first and last directions of the ray will be parallel to a plane containing the intersection of *a*, *c*, and will make equal angles with the intersection of *a*, *c*, which is also a normal to the face of the mirror. Therefore, if two parallel rays fall, one on the mirror, and one on either of the planes *a*, *c*, the direction of the ray reflected from the mirror will be parallel and opposite to that of a ray reflected once at each of the planes *a*, *c*. When the small reflectors are made of bits of unsilvered glass, the brightness of the image of the sun is so far reduced after the second reflexion, as not to interfere with the direct vision of T, and the mirror can be pointed without difficulty.

The other consists of a plate of glass having parallel faces *b*, *d*, with two polished plane faces *a*, *c* on its edges, making right angles with one another, and with the faces *b*, *d*, the face *d* being silvered, with the exception of a portion at the angle *adc* not larger than the pupil of the eye. It is easily seen that if a ray of light incident upon *b*, and refracted

through *b*, so as to be reflected internally once at each of the planes *a*, *c*, emerge through *d*, the planes of incidence and emergence will be parallel, and the incident and emergent rays will make equal angles with the edge *ac*, and therefore with a normal to the faces *b*, *d*. Hence the portion of the incident ray which is reflected from the mirror will proceed in a direction parallel and opposite to that portion of the ray which, after internal reflexion at *a* and *c*, emerges through *d*.

In order to ascertain that the construction of such an instrument presented no unforeseen difficulties, I requested Mr. T. E. Butters, of 4, Crescent, Belvedere Road, the well-known maker of sextant-mirrors and artificial horizons, to form the faces *a*, *c* on the edges of a piece of plate glass, and then had the face *d* coated with chemically reduced silver. Upon trial, the emergent light was found to be too bright; but after smoking the angle *adc* in the flame of a candle, in order to reduce the intensity of the light, it became perfectly easy to make the centre of the image of the sun coincide with the object T seen by direct vision.

An image of the sun of suitable intensity for pointing might be obtained by attaching to the edge of the mirror a piece of tinted glass, of the form of the corner *abcd*, with the faces *b*, *d* parallel to the plane of the mirror.

The Society then adjourned, over the Whitsuntide Recess, to Thursday, June 15, the President having announced the Meeting for the Election of Fellows to take place on Thursday, June 1, at 4 P.M.

June 1, 1865.

The Annual Meeting for the election of Fellows was held this day,

Major-General SABINE, President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Mr. Brayley and Dr. Webster were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society.

The Hon. James Cockle, M.A.
Rev. William Rutter Dawes.
Archibald Geikie, Esq.
George Gore, Esq.
Robert Grant, Esq., M.A.
George Robert Gray, Esq.
George Harley, M.D.
Fleeming Jenkin, Esq.
William Huggins, Esq.
Sir F. Leopold McClintock, Capt. R.N.
Robert McDonnell, M.D.
William Kitchen Parker, Esq.
Alfred Tennyson, Esq., D.C.L.
George Henry Kendrick Thwaites, Esq.
Lieut.-Col. James Thomas Walker, R.E.

Communication from the President and Council of the Royal Society to he Board of Trade on the subject of the Magnetism of Ships*.

"To the Right Hon. Thomas Milner Gibson, President of the Board of Trade.

"The Royal Society, May 18, 1865.

"SIR,—The attention of the Fellows of the Royal Society has been recently directed to the very great increase which has taken place in the employment of iron in the construction and equipment of ships, and the consequent augmentation of the embarrassment occasioned in their navigation by the action of the ship's magnetism on their compasses.

"The inconveniences which have already made themselves felt in the ships of the mercantile marine, and which threaten to be productive of very serious loss of life and property, unless remedial measures be adopted similar to those which have proved so advantageous to the ships of Her Majesty's Navy, have induced the President and Council of the Royal Society, after much consideration, to venture on the step of calling your attention, as presiding over the Department of Trade, to a subject which they believe to be of pressing importance.

"In this view the accompanying Memorandum has been prepared, stating, as briefly as may be, the particulars which they are desirous of bringing under your consideration; in the belief that the time has fully arrived when measures of a more stringent and effectual character are required, in the direction which has been already taken by Her Majesty's Government in such legislative enactments as those contained in the Merchant Shipping Act (1854), adverted to in the accompanying Memorandum.

"I have only to add that it would afford the President and Council great satisfaction if they could be of any further assistance in a matter which they believe to be of so much importance.

"I have the honour to be,

"Your obedient Servant,

"EDWARD SABINE,

"*President of the Royal Society.*"

"Memorandum.

"It is believed that the time has come when it is expedient that the Executive Government should exercise a more direct and systematic supervision over the adjustment of the compasses of ships of the mercantile marine than it has hitherto done. The opinion that it might do so with advantage is not new, as may be seen from passages in the 2nd and 3rd Reports of the Liverpool Compass Committee (2nd Report, p. 30; 3rd Report, p. 38), but it has of late been gaining strength from the following among other circumstances:—

* Published in the Proceedings, by order of the Council.

"(1) The great increase in the number of iron ships, as well as in the amount of iron used in the construction of such ships.

"(2) The losses of iron ships.

"(3) The advances which have been made in, and the present state of, the science of the deviation of the compass.

"We may consider these separately.

"1. It is believed that for some years the number of iron ships constructed has greatly exceeded that of wood-built ships, and this is particularly the case as regards passenger steamers. In such vessels iron is now used not only in the construction of the hull but in decks, deck-houses, masts, rigging, and many other parts of the ship for which wood was till recently used. The consequence has been a great increase in the amount of the deviation of the compass, increased difficulty in finding a proper place for the compass, and increased necessity for, and difficulty in, applying to the deviation either mechanical or tabular corrections.

"2. Many recent losses of iron steamers have taken place, in which it is probable that compass-error has occasioned the loss. In most of these, however, from the want of any record of the magnetic state of the ship, of the amount of original deviation, and of the mode of correction, and from the investigations into the causes of the loss being conducted by persons not instructed in the science, and who are necessarily incompetent either to elicit the facts from which a judgment can be formed, or to form a judgment on those facts which are elicited, no certain conclusion as to the cause of loss can be arrived at. The investigations are, however, sufficient to show the want of a better and more uniform system of compass-correction in the mercantile marine, and of more knowledge of the subject among masters and mates.

"3. Since the first introduction of iron ships it has been a recognized fact that they cannot be safely navigated without the compass being as it is termed 'adjusted,' *i. e.* without the deviations being corrected either mechanically by magnets or by a table of errors; but at first the correction of each ship was a separate and independent problem. Now the case is different. The theory of the deviation, its causes, and its laws, are now thoroughly understood and reduced to simple formulæ, leaving the numerical magnitude of a certain small number of quantities to be determined by observation for each ship separately; and further, by recording, reducing, and discussing the deviations which have been observed in the ships of the Royal Navy of different classes, numerical results, as to the values of these quantities in ships of each class, have been determined which promise to be of the greatest use in facilitating the complete determination of the deviation and its correction, and in suggesting modes for constructing iron ships, and in the selection of the position of the standard compass. The science of magnetism, in its relation to navigation, is, in fact, in a position in some degree analogous to that in which the science of astronomy at one time was. The principles of the science have been

established, the formulæ have been obtained; but numerical values are wanted, which can only be derived from a large number of observations systematically made and discussed. At present these numerical results have only been obtained from, and are only applicable to, the ships of the Royal Navy. Without some systematic direction, the mercantile marine can neither derive the full benefit of, or contribute its due share to, the advance of the science.

"That the subject is one coming properly within the cognizance of the Board of Trade may be inferred from the Legislature having already in the Merchant Shipping Act, 1854, sect. 301, art. (2), provided that 'every sea-going steamship employed to carry passengers shall have her compasses properly *adjusted* from time to time, such adjustment to be made to the satisfaction of the Shipwright Surveyor, and according to such regulations as may be issued by the Board of Trade.' The Shipwright Surveyor is then (sect. 309) to make a 'declaration' that the 'compasses are such and in such condition as required by the Act,' and on such 'declaration' the 'certificate' of the Board of Trade is issued.

"It does not appear how these enactments are construed or carried into effect. It is not, however, understood that the Shipright Surveyor is expected or is necessarily competent to do more than see that the ship is furnished with proper compasses, but the goodness of the compass has nothing to do with the deviation; the best compasses are affected by the deviation precisely in the same way and to the same extent as the worst*. It is not understood that he exercises any judgment or control as to the position of the compass, the amount of deviation, or the mode of adjustment, or any of the various points which are involved in the compass being 'properly adjusted.'

"As regards the important subject of 'deviation,' all that has been done by the Board of Trade consists, it is believed, in the publication of the 'Circular on Deviation' compiled by Admiral FitzRoy, the publication of the Reports of the Liverpool Compass Committee, and the publication of 'Practical Information for Masters and Mates,' by Mr. Towson.

"As regards the particular points to which the attention of the Board of Trade may be invited, they may be considered under the following heads:—

"(1) The correction of the compass in particular ships.
"(2) The advancement of the science of deviation of the compass.
"(3) The education of Masters and Mates.

"1. As before observed, it is now recognized that every iron ship must have its compasses 'adjusted.' Hitherto two totally different modes of

* This is subject to the qualification that, from the diminution of directive force in ships having large deviations, compasses of superior power and delicacy are required; and if the compasses are corrected by magnets, a particular arrangement of needles is requisite.

adjustment have been practised, each of which has its advantages and disadvantages.

"1. The system recommended by a Committee of Men of Science and Naval Officers appointed by the Admiralty in 1837, and which has been uniformly followed in the Royal Navy from that time. In this system each ship has a 'Standard Compass,' distinct from the Steering-Compass, fixed in a position selected, not for the convenience of the steersman, but for the moderate and uniform amount of the deviation at and around it. The ship is navigated solely by that compass. The deviation of that compass on each course is ascertained by the process of 'swinging' the ship; a table of deviation is formed, and the deviations given by the tables are applied as corrections to the courses steered.

"2. The system proposed by the Astronomer Royal in 1839, and which is understood to be generally followed in the mercantile marine. In this system the deviations of the compass are compensated by magnets (and occasionally soft iron). The ship is navigated by the compass so corrected—generally the steering-compass, and generally without any tabular correction.

"It would not be right, considering the weight of authority on each side, to pronounce any decided opinion against either of those modes of correction when properly used. The first system has proved in the Royal Navy to be one which can be used without danger. The same cannot be said of the second method as regards the mercantile marine; but the principal danger of the method arises from what is in truth an abuse of the method: it is that, in reliance on the power of correcting any amount of original deviation, however great, the navigating-compass is placed in a position in which the original deviations are excessive and vary rapidly, and in which no navigating-compass should be placed.

"In merchant ships the most convenient place for the steering-compass is generally near the upper part of the stern-post, the rudder-head, the tiller, and the iron spindle of the steering-wheel—all, from their shape and position, powerfully magnetic. The constructor and owner, for the sake of economy, desire that the steering-compass should be the navigating-compass. The compass-adjuster fears that any objection on his part would be considered a confession of incompetence, and that some less scrupulous adjuster would not hesitate to undertake the correction. The correction can only be made by powerful magnets. The compass is then held, as it were, in equilibrium by powerful antagonistic force; and when the changes take place, which it is known do take place in all new iron ships, or when any changes take place in the magnets, large errors are introduced, which are the more fatal because the shipmaster is taught to believe that his compass is correct.

"This abuse of the method is one the temptation to which is unfortunately so strong, that it is believed it can only be effectually prevented by prohibiting the use of the steering-compass as the navigating-compass, or

rather by requiring that the ship shall have a navigating-compass distinct from, and in addition to, the steering-compass.

"It is therefore recommended that every iron passenger-ship should be required to have a standard compass distinct from the steering-compass in a selected situation at a certain distance from all masses of iron; that, whether corrected or not, the original deviations of the standard compass should not in ordinary cases exceed a certain limited amount; and that on each occasion of the compass being adjusted, a table of the deviations should be furnished to the Master and returned to the Board of Trade; and that if corrected by magnets, a return should be made of the position of the magnets and of every subsequent alteration of their position. Provision may be made for exceptional cases, in which it may be found impracticable to place the standard compass in a position where the original deviation is within the limit, by requiring in such cases a special certificate from the central authority.

"It may be here observed, as regards many practical matters connected with the adjustment of the compass in particular ships, in which at present great diversity of practice prevails, that an organized department under a skilful superintendent in constant communication with the ports, would probably be of the greatest service, not merely in laying down rules, but in giving advice and suggestions to naval constructors, compass-makers and adjusters, and producing a uniform system of adjustment at the different ports, which would be generally understood by shipmasters. Advice from the same source would be not less useful to the authorities in the different ports in suggesting means of facilitating the adjustment by meridian-marks on shore, laying down moorings, &c. It would probably be one of the first duties of the superintendent of such a department to acquaint himself thoroughly with the methods practised at the different ports, and to give such suggestions, either in the form of reports to the Board of Trade, or in private communications, or both, as might appear to him advisable. Such a superintendent would also be available as an assessor in investigations into the loss of iron vessels, in cases in which there is any possibility of the loss having been occasioned by compass-error.

"2. The advancement of the science of the Deviation of the Compass.

"Whatever difference of opinion exists as to the advantage or necessity of a Standard Compass as regards the safety of particular ships, there is none as to its being indispensable for any scientific inquiry into the amount of the deviation and of its constituent parts and its changes. It is from the Tables of the deviation of such compasses, and such compasses alone, observed at different times and places, and systematically reduced and discussed, that those numerical results can be obtained which promise to be so useful in securing in iron ships a place for the Standard Compass where the deviation is of a safe and manageable amount, and in guarding against the dangers which arise from changes in the magnetism of recently built ships. It is from the recorded deviations of such compasses that on the loss of a

ship a judgment may be formed of the effect of the deviation in causing any error in the course of the ship.

"3. The education of Masters and Mates.

"At present it may be said that entire ignorance of the subject is the rule.

"The subject has not hitherto been a recognized branch of the education of the seaman; and the most skilful seamen frequently either ignore it altogether, or look upon it as a mystery not capable of comprehension. Now, however, that the principles of the science have been established, it is found that the subject is not one of any serious difficulty; and although it might not be considered just to require Masters and Mates already certificated to pass an examination in a new subject, yet an opportunity might be given them of passing a voluntary examination; and as regards future Candidates for a Certificate of competence, notice might be given that after a certain period, say two or three years, a certain amount of knowledge of the subject will be required from Candidates (and in the mean time a text-book containing the necessary amount of information might be prepared and published), and the Examiners of the Local Marine Boards will themselves receive instruction, and, if necessary, undergo an examination on the subject.

"For the purposes indicated, it seems desirable to establish a department of the Board of Trade under a competent Superintendent, the whole, or greatest part of whose time should be devoted to this subject. Almost all the advances which have hitherto been made in the science, and which have placed England at the head of the science, are due to there having been for the last twenty-five years one Officer charged by the Admiralty with this duty almost exclusively. Such an Officer becomes the depositary of all that is known on the subject, and has no difficulty in obtaining the best scientific assistance. It seems desirable that for some years at least the Board of Trade should take advantage of the ability and experience of the present Superintendent of the Compass Department of the Navy. It is understood that there would be no practical difficulty, and there would be many advantages in the present state of the science in having the superintendence of the compasses of the Royal and Mercantile Marine united in one head, with competent assistants in the two branches of the service. The subject, as has been observed, is not one of difficulty. Any intelligent man could speedily be instructed in all that would be necessary to enable him to discharge the duties of Assistant for the Mercantile Marine; and in the selection of such an Assistant, probably it would be more important to look to general ability, intelligence, docility, and the habit of, and aptitude for, dealing with men, and particularly with Masters of merchant vessels, than to any previous knowledge of the subject."

Correspondence between the Board of Trade and the Royal Society in reference to the Meteorological Department*.

From T. H. Farrer, Esq., Marine Secretary of the Board of Trade, to General Sabine.

"Board of Trade, Whitehall,
26 May, 1865.

"Sir,—I am directed by the Lords of the Committee of Privy Council for Trade, on the occasion of the vacancy in the Office of Chief of their Meteorological Department, caused by the untimely death of Admiral FitzRoy, to request you to be so good as to bring under the notice of the President and Council of the Royal Society the correspondence which took place between that Society and this Office at the time of the institution of the Meteorological Department as a branch of this Office; and particularly your letter of the 22nd February, 1855†, in reply to that from this Office of the 3rd of June, 1854, in which, when about to institute the Department, My Lords had desired the opinion of the Royal Society as to what were the great desiderata in Meteorological Science. The recommendations of the Royal Society conveyed in your letter of the 22nd of February, 1855, were adopted as the basis of the proceedings of the Meteorological Department; instruments were provided, logs were prepared, furnished, and returned to the office, and some progress was made in carrying into effect the original programme.

"But in 1859 or 1860, the French Government having adopted a system of telegraphing and publishing the actual state of weather from one place to another, cooperation in which was urged on the Board of Trade by a Committee of the British Association and by Admiral FitzRoy, My Lords gave their sanction to what was proposed, and thenceforward a considerable part of the vote previously applied to obtaining and digesting observations was diverted to these Telegrams. In 1861 Admiral FitzRoy grafted on this system of telegraphic communication a system of forecasting the weather (the forecasts being published in the daily papers), and, on occasions of anticipated storms, the giving of special warnings, communicated by telegraph to the different Ports, and there made known by hoisting certain signals. The whole, or almost the whole, of the Funds originally voted for the purpose of observations were thus diverted from their original scientific object to an object deemed more immediately practical.

"In 1863, on the occasion of an increased estimate for the purpose of these forecasts, it was determined to compare the forecasts and the warnings with the actual results.

"As regards the daily forecasts, the daily reports of weather published by Admiral FitzRoy afforded, and still afford, ample means of checking them.

"As regards the storm-warnings, detailed reports were called for from

* Published by order of the Council.

† Proceedings, vol. vii. p. 342.

the places to which the warnings were sent. The results of these comparisons for certain periods were tabulated and laid before Parliament in a paper, copy of which is annexed. The data for continuing the return are still kept, and, if it were thought right to incur the expense, it could be continued at any time.

"My Lords at the same time addressed a further letter, dated 27th February, 1863, asking the opinion of the Royal Society as to the course then being pursued by Admiral FitzRoy, and were favoured in reply by your letter of the 27th March, 1863.

"The vacancy in the Meteorological Department, occasioned by the death of Admiral FitzRoy, has seemed to My Lords to present a fitting opportunity to review the past proceedings and present state of the Department; and with this view they are desirous of receiving any observations or suggestions, with which the President and Council of the Royal Society may be willing to favour them, on the constitution and objects of the Department, and the mode in which those objects may be most effectually attained.

"The points on which the Board of Trade especially desire the opinion of the Royal Society are the following:—

"1. Are the objects specified in the Royal Society's letter of the 22nd February, 1855, still as important for the interests of Science and Navigation as they were then considered?

"2. To what extent have any of these objects been answered by what has already been done by the Meteorological Department?

"3. What steps should be taken for making use of any observations already collected, or any compilations already made by the Department?

"4. Is it desirable to make any, and what, further observations on any, and which of the subjects mentioned in the Royal Society's letter of 22nd February, 1855?

"5. What is the nature of the basis on which the system of daily forecasts and storm warnings established by Admiral FitzRoy rests? In other words, are they founded on scientific principles, so that they, or either of them, can be carried on satisfactorily notwithstanding Admiral FitzRoy's decease?

"6. If they, or either of them, can be carried on satisfactorily, can the Royal Society suggest any improvement in the form and manner of doing it?

"7. Is it desirable to continue down to the present time the tables of results corresponding to the forecasts and storm warnings which were made out for certain periods in the year 1863, and were presented to Parliament in April 1864 (Parliamentary Paper, No. 200, Session 1864, inclosed)? The materials for doing this exist in the Office, and only require clerical labour.

"8. Assuming it to be desirable to continue the publication of the daily reports of weather received from various stations, can the Royal Society

make any suggestions as to the extent to which it should be carried, and the form in which it should be done?

"9. Have the Royal Society any general suggestions to make as to the mode, place, or establishment in, at, or by which the duties of the Meteorological Department can best be performed?

"With respect to these heads of inquiry, My Lords desire to observe, in the first place, that they understand that the Admiralty are willing to undertake, and to place in the hands of their Hydrographer, all those observations which can properly be made use of in framing charts for purposes of Navigation, but not those which relate to Meteorology proper.

"Secondly. That the Board of Trade will gladly place the knowledge and services of Mr. Babington, Admiral Fitzroy's second, at the disposal of the Royal Society, for the purpose of the above inquiries, and will also give them any help, clerical or otherwise, which the Royal Society may require, and which the Board of Trade may be able to give.

"I have the honour to be,
"Sir,
"Your obedient Servant,
"T. H. Farrer."

"*The President of the Royal Society.*"

Report by Mr. Babington *on Forecasts and Storm-warnings, communicated with* Mr. Farrer's *Letter.*

"Meteorological Department,
May 11, 1865.

"The following is an attempt to comply with a request from Mr. Farrer for some explanation, in a few words, of the method adopted in this Department with regard to Forecasts and Storm Warnings—the basis on which forecasts have been made and cautions given.

"Admiral FitzRoy has devoted three chapters (XIII., XIV., XV.) of his 'Weather Book' to this particular subject. I do not think the matter can be thoroughly comprehended without reference to those chapters. The very brief explanation which follows here must necessarily be most incomplete.

"About ten o'clock each morning (except Sundays) telegrams are received here from about eighteen places round our own coasts, from a few French ports, and from Heligoland. These telegrams report (in cipher, for brevity) the state of the atmosphere, including pressure, temperature, wind direction and force, degree of dryness, rainfall, state of sky and sea, at each station.

"The observations thus telegraphed are immediately reduced, or corrected, for scale-errors, elevation, and temperature, and are written into prepared forms.

"The first copy, with all the telegrams, is passed to the chief of the department, or the person appointed by him, to be studied for that day's

forecasts. At eleven, copies of the report, with forecasts, are sent out to 'The Times' (for second edition), to the 'Shipping Gazette,' and to the principal afternoon papers. Copies of the forecasts, only so far as they relate to weather expected in the Channel and on the French coasts, are telegraphed to Paris (by special request) for the Ministry of Marine. The whole of this work is finished by about half-past eleven, when every one in the department is free to turn his attention to other duties. Late in the afternoon telegrams are received from a very few selected stations. Should it appear necessary (which is now but seldom), in consequence of this later information, the morning's forecasts are more or less modified, and copies of the report are sent out for the next morning's early papers.

"Besides this daily service, *occasional* storm warnings, or cautions, are sent to our own coasts and to Paris, and, when it appears advisable, also to Hamburg, Hanover, and Oldenburg, by the request *and at the expense* of the Governments of those States.

"The basis upon which the forecasts and the cautions (which are merely forecasts symbolized) are founded, may be stated briefly as follows:—

"They are the result of theory and experience combined. They are not *predictions* but *opinions*, although probably the *best* opinions that can be formed; for it is manifest that if we know what is and *has* been occurring around an area several hundred miles in diameter, we are in a better position to form an opinion respecting the probable weather in a particular district than any person who has not such facts at command.

"Considering, with Dove, that there are two constant principal wind-currents, north-east and south-west, of which the characteristics, especially with regard to temperature and degree of moisture or dryness, are totally distinct, all varieties of wind and weather in these latitudes may be traced to the operation of these two main currents—singly, in combination, or in antagonism—at times running in parallel lines but in opposite directions, frequently *superposed*, and occasionally meeting at various angles of incidence.

"Upon the relative prevalence or failure of either or both of these currents all conditions of weather appear to depend.

"It is clear that changes must begin at some places earlier than at others; and the observations telegraphed daily to this department from the outports afford the means of forming a very good opinion respecting the nature and probable course of such changes. In a paper of this kind one or two examples must suffice, although the various examples that might be given are numerous, as also are the disturbing causes which must be taken into account by a *Forecaster*.

"Suppose a northerly (E.N.E.–N.N.W.) current to have been prevailing generally over this country with *fine* weather; the barometers at all the outports, during the continuance of such weather, will have been steady, or slowly rising, nearly uniform, or slightly higher at the northern than at the southern stations; there will have been much evaporation, and

the sky will have been comparatively clear and free from cloud, while the temperatures, when free from the influence of radiation, will have been somewhat below the average.

"Suppose now the northern barometers rise rapidly above the average, while the temperature remains low and the sky clear. This is an indication of *more* wind from a northern quarter, but probably without rain. Should, however, the southern barometers fall at the same time, while the temperatures in the south are much higher than in the north, the wind will probably increase to a *gale* from a northern quarter, and a sudden chilling of the atmosphere in the south will ensue, causing rain.

"The first approach of a southerly current is usually indicated by a diminution of pressure (falling barometers) in the north and west, caused by a failure of the polar current; the upper clouds are seen to be passing from the south, and the temperature increases.

"Occasionally a temporary failure of both currents takes place. We may have very low barometers, but for a day or two little or no wind. Such a state of things, however, never continues long. It is then especially necessary to watch for the first signs of approaching wind. The first indication is usually an *increase* of pressure in the direction from which the wind is coming. Should the French barometers rise rapidly or stand (say) an inch higher than in Scotland, the result will be a gale from the southward. Should *both* the French and the Scotch barometers rise rapidly, while in Ireland and central England the pressure continues very low, we may feel sure that *both* currents are approaching in force, and that the collision will be violent, causing much rain, and (according to the angle of incidence) either south-west and north-west gales, or a cyclonic movement, which experience has shown will probably advance in an easterly or north-easterly direction. On the other hand, should the increase of pressure be gradual and general, the combination of the two currents will be gradual also, and, though there may be rain, the winds will not be violent.

"There are a few grand rules, which, though not always free from disturbing causes, may be considered as generally holding good.

"1st. The essentially distinct characteristics of the two main currents should never be forgotten.

"2nd. The direction of wind is *usually* from the place of high barometer towards the region of low barometer.

"3rd. The *force* of wind is usually proportional to the *differences* of barometric pressure, *not* (as has been asserted by some) to the actual pressure. It matters little how low the barometer may be, if it is equally low for a considerable distance around. In such a case wind cannot follow at once, for there is no available supply at hand.

"4th. It was believed by Admiral FitzRoy that there exists a lateral transference of the whole body of atmosphere eastward.

"Electrical and auroral occurrences should be carefully watched; and the influence of high land, &c., must be borne in mind.

"There is another point in connexion with forecasting in which Admiral FitzRoy took great interest, namely, the frequency with which important atmospheric disturbances have been preceded by disturbances on electric wires above ground, and also on *submarine* wires.

"No argument, or opinion, with regard to the advisability or otherwise of the continuance of the present system of forecasting is offered here, because none was asked for. I may mention, however, that the system, though at first objected to at the Paris Observatory, has *since* been adopted at that place, but that nevertheless the *London* forecasts are still sent daily to the French Ministry of Marine at the request of that department.

"T. H. B."

From General Sabine to Mr. Farrer.

"Royal Society, Burlington House,
"June 15, 1865.

"Sir,—In replying to your Letter of the 26th of May, the President and Council think it may be desirable to advert in the first instance to that which has constituted the chief occupation of Admiral FitzRoy's Department in the last four or five years, viz., the systematic forecasting of the weather by means of telegrams received from stations comprised within a certain limited area, and, on occasions of anticipated storms, the giving special warnings conveyed by telegraph to the different ports in the United Kingdom, and there made known by hoisting certain signals.

"The system of forecasting which Admiral FitzRoy instituted and pursued has been expressly described by himself as 'an experimental process,' based on the knowledge conveyed by telegraph of the actual state of the winds and weather and other meteorological phenomena within a specified area, and on a comparison of these with the telegrams of the preceding days, so as to obtain inferences as to the probable changes in the succeeding days. The proper test of the efficiency and usefulness of such a system of cautionary signals at the different ports is to be sought in the *measure of success which it appears to have attained*—always remembering that the system under consideration can only be regarded as in its infancy, and that, if continued, its improvement, and consequently its importance, may be expected to be progressive from year to year. In Admiral FitzRoy's Report to the Board of Trade in May 1862, the opinions of the shipmasters at several ports in regard to the practical value which they attached to the storm-signals were given at length. Of the 56 replies published in the Appendix of that Report, 46 were decidedly favourable, 3 decidedly unfavourable, and 7 expressing no decided opinion. A statement so favourable on the whole, obtained so very shortly after the system had been first brought into operation, must surely be considered to have fully justified the Board of Trade in directing its further prosecution.

"The return to the House of Commons, dated April 13th, 1864, a copy of which accompanied your letter, presents a comparison of the probable force of the wind as indicated by the signals in the year commencing

April 1st, 1863, and terminating March 31st, 1864, and its actual state, as reported in the three days following the exhibition of the signals; and Mr. Babington has since been so obliging as to communicate in manuscript a return having the same object in view for the year April 1st, 1864, to March 31st, 1865.

"From the first of these documents, the President and Council learn (in p. 7) that the whole number of signals which were hoisted at different places, and of which reports were received, between April 1, 1863, and March 31, 1864, amounted to 2288; of these the number which proved correct in respect to the *Force* of the wind equalling or exceeding 'a fresh gale,' was 1284; in 462 cases the stations were reached by the gale (or a still stronger wind blew) before the signal was hoisted; and in 726 within forty-eight hours after the signal was hoisted. Hence we may conclude that (omitting the 96 cases in which the gale occurred between 48 and 72 hours after the signal was hoisted) 1188 signals, or more than half the whole number of 2288, were justified by the state of the weather, either when the telegraphic message reached the station, or within forty-eight hours afterwards.

"With respect to *direction* of wind in a gale indicated by signal, the 'warnings' are reported to have been much less frequent. Of the 402 signals indicating direction as well as force, 271 agreed, and 131 did not agree with the real direction of the wind—being a proportion of about two correct to one incorrect.

"The manuscript with which Mr. Babington has favoured the Council since the receipt of your letter of May 26, 1865, contains a summary of the cautionary signals between April 1, 1864, and March 31, 1865, with notes stating their success or failure. From these it appears that signals were hoisted on 40 days in the course of the year, 29 of which appear to have been justified by the event, 8 to have been failures either in respect to force or direction, and 3 were late, the gale having already commenced. There are also 5 cases in which it is admitted that signals might have been made with advantage when none were sent.

"It seems not unreasonable to attribute to increased experience the marked improvement of these results upon those of the preceding year, and to anticipate still further improvement.

"The method adopted in preparing the storm-warnings has been very ably and lucidly explained by Mr. Babington in a paper dated May 11, 1865, presented by him to Mr. Farrer, by whom a copy has been sent to the President and Council. Possibly it may be viewed as the best arrangement that this branch of the duties of the office shonld continue as at present under the direction of Mr. Babington, by whom it has been virtually carried on for several months past.

"On the subject of storms of a cyclonic character originating in the British Islands or in their vicinity, the interest of which was adverted to in the reply from the Royal Society to the Board of Trade, March 27, 1863,

reference has been made to Mr. Babington for such further information as may have been subsequently obtained. His reply to General Sabine is as follows:—

"'I can quite confirm your impression respecting Admiral FitzRoy's belief in the evidence of the existence of small cyclonic storms in England itself, originating in or near our islands, and generated in the brushing against each other of the N.E. and S.W. currents; and in reply to your question I beg to say that I believe there is satisfactory evidence of the existence of such storms, but that these small storms are not very frequent—three or four in a year perhaps—and that they are, I think, more common in summer than in winter, although usually of less violence. The *direction* of their motion is certainly almost invariably towards some point between N.N.E. and E.S.E. With regard to the rapidity of their motion, I scarcely feel able to express an opinion; but at the ordinary rate of progression it takes such a storm about forty-eight hours to pass from Ireland to the Baltic. Not unfrequently, however, they appear to die out (as it were) before travelling so far.'

"The existence of such storms in our islands is a fact in meteorological science of considerable interest, for which we are indebted to the researches instituted and carried on by Admiral FitzRoy's department. Though not of very frequent occurrence, they constitute a class of phenomena well suited for telegraphic advertisement, especially on our eastern and north-eastern coasts. It might perhaps be practically desirable to indicate them by a special signal, distinguishing them from storms which have a more uniform direction. But however this may be, it seems to be desirable that the occurrence of such storms and their attendant phenomena, as obtainable at the time, should be carefully recorded, with a view to the records being ultimately put together in elucidation of a branch of the meteorology of our islands which has hitherto been but imperfectly examined.

"We proceed to notice the points on which we are informed that the Board of Trade especially desire the opinion of the Royal Society—and particularly the inquiry whether the objects specified in the Royal Society's letter of the 22nd of February 1855 are still viewed as of the same importance for the interests of science and navigation as they were then considered.

"The most prominent amongst these objects was the collection and co-ordination of meteorological observations made at sea, including such as are required to form a correct knowledge of the currents of the ocean, their direction, extent, velocity, and the temperature of the surface water relatively to the ordinary ocean temperature in the same latitude, together with the variations in all these respects which currents experience in different parts of the year and in different parts of their course. These—as well as the facts connected with the great barometric elevations and depressions which we know to exist in several oceanic localities, and their influence on circumstances affecting navigation—were noticed as inquiries well de-

serving the attention of a country possessing such extensive maritime facilities and interests as ours, and as forming a suitable contribution on our part to the general system of meteorological inquiry which had been adopted by the principal continental states in Europe and America.

"We have learnt from Mr. Babington that much was done by Admiral FitzRoy in the three or four years succeeding the establishment of his office (and before the subject of storm-warnings had engrossed the greater part of his consideration), in directing the attention of many of the commanders of our merchant ships to the collection of suitable data, and in improving their habits of observation and of record. The logs of such vessels form at present a large collection of documents existing in the Office of the Board of Trade, partially examined, and their contents partially classified. The President and Council are glad to learn by your letter that the further prosecution of this great and important branch of Hydrography is about to be placed in the hands of the distinguished officer who now presides over the Hydrographic Department of the Admiralty; to whose duties it appears indeed most appropriately to belong, and to whose office no doubt the documents already collected will be transferred and made available for public purposes.

"There remain, therefore, to be noticed solely the considerations which relate to 'Meteorology proper,' *i. e.* to the Land Meteorology of the British Islands. We find that the principal States of the European continent have almost without exception formed establishments for the collection and publication periodically of the meteorology of their respective countries. The arrangements consist usually of a central office, at which instruments and instructions are provided for a number of stations, greater or less, according to the area which they represent; at which stations observations are made and transmitted to the central office, where the results of all are reduced, coordinated, and published. The small extent of the area comprised by the British Islands in comparison with the territories of many of the European States, may require *fewer* stations; but in a matter now so generally attended to and provided for, it seems scarcely fitting that our country should be behind others. There is, moreover, a peculiarity in the meteorological position of the British Islands in respect to Europe generally as its north-western outpost, in consequence of which an especial duty appears to devolve upon us. M. Matteucci, in a very recent publication, has already made the important remark that extensive atmospheric disturbances which first invade Ireland and England are those which, in winter more especially, extend to and pass the Alps (although somewhat retarded by them), and spread over Italy—and thus that, though receiving telegrams announcing storms taking place in the North of Europe, in Germany, on the western coasts of France, and of those of Spain, he finds that it has in fact been most especially in the case of announcements from England that storms so telegraphed have actually

reached Italy, and been found to correspond with the accounts subsequently received from Italian Mediterranean ports.

"A few stations,—say six, distributed at nearly equal distances in a meridional direction from the south of England to the north of Scotland, furnished with *self-recording* instruments supplied from and duly verified at one of the stations regarded as a central station, and exhibiting a *continuous* record of the temperature, pressure, electric, and hygrometric state of the atmosphere, and of the force and direction of the wind—might perhaps be sufficient to supply authoritative knowledge of those peculiarities in the meteorology of our country which would be viewed as of the most importance to other countries, and would at the same time form authentic points of reference for the use of our own meteorologists. The scientific progress of meteorology from this time forward requires indeed such continuous records, first, for the sake of the knowledge which they alone can effectively supply, and next, for comparison with the results of independent observation not continuous. The actual photograms, or other mechanical representations, transmitted weekly by post to the central station would constitute a lithographed page for each day in the year, comprehending the phenomena at all the six stations, each separate curve admitting of exact measurement from its own base-line, the precise value of which might in every case be specified.

"The President and Council suggest that the Observatory of the British Association at Kew might, with much propriety and public advantage, be adopted as the central meteorological station. It already possesses the principal self-recording instruments, and the greater part of them have been in constant use there for many months. There will be no difficulty in obtaining, through the intervention of the Committee of Management, similar instruments for the affiliated meteorological stations, and in arranging for their verification and comparison with the Kew standards, as well as in giving to those in whose hands they may be placed such instructions as may ensure uniformity of operation. The records from the other stations may be received at Kew by post weekly, or more frequently if required, and may be at once arranged for such form of publication as may be most approved. It seems expedient that, if practicable, the stations which should be selected to act in concert and cooperation with Kew should be in localities where some permanent establishment of a scientific character exists, and where a certain amount of supervision may be secured. In this view the President and Council would suggest, as eligible, the following chain of stations, commencing from the south, viz. :—

		°	′
Falmouth—Polytechnic Institution	Lat.	50	9
Kew—Observatory of the British Association	„	51	28
Stonyhurst—The College, which has already a magnetical and meteorological observatory	„	53	0
Armagh—Observatory	„	54	21

Glasgow—University and Observatory	Lat.	55°	51′
Aberdeen—University..........................	„	57	9

"To these six stations the President and Council would have been very glad to have added two others, one in the south-west and one in the north-west of Ireland. For the former of these, possibly Valentia may present a fitting locality, when an establishment shall have been formed there as the connecting link, by means of the Atlantic Telegraph between Europe and America.

"Having answered thus generally, it may perhaps be desirable to add specific replies on the several points enumerated in Questions 1 to 9. Preserving the order in which the inquiries are made, the replies are as follows:—

"*Question* 1. The President and Council are of opinion that the objects specified in the Royal Society's letter of February 22, 1855, are as important for the interests of science and navigation as they were then considered.

"*Question* 2.—Much has without doubt been accomplished in the collection of facts bearing on Marine Meteorology, but as no systematic publication of the results has yet been made, the President and Council are unable to reply more specifically.

"*Question* 3.—The President and Council recommend that the Sea Observations should be placed in the hands of the Hydrographer, with a view to the introduction of the results into the Admiralty Charts. They, however, at present have not sufficient information on the subject of the Land Observations which may exist in the office of the Board of Trade to justify them in offering any recommendation thereon.

"*Question* 4.—The President and Council consider it very desirable that further observations should be made, especially with reference to oceanic currents and great barometric depressions, and generally on all subjects comprehended under the denomination of 'Ocean Statistics.'

"*Questions* 5 & 6.—It appears from the late Admiral FitzRoy's reports, as well as from the explanations of Mr. Babington, that the storm-warnings have been based on inferences drawn from observations extending over a considerable area; and the President and Council recommend that they should be continued under the superintendence of that gentleman. Respecting the daily forecasts of weather, however, they decline expressing any opinion.

"*Question* 7.—The President and Council are of opinion that it would be desirable that an annual report in a modified form should be made to the Board of Trade of the results from the storm-warnings in the preceding year, and should be communicated to Parliament, and thereby become known to the public.

"*Question* 8.—A proper reply to this question would require information and involve considerations which would occasion an inconvenient delay in the transmission of this letter.

"*Question* 9.—The suggestions of the President and Council in regard to the mode in which it appears to them that the important subject of 'Meteorology Proper,' or the 'Land Meteorology of the British Islands,' might be dealt with economically, and at the same time effectively, have been fully stated in the body of this letter.

"I have the honour to be, Sir,
"Your obedient Servant,
"EDWARD SABINE,
"*President of the Royal Society.*"

Extracts from a Letter to the President from Professor Dove, *of Berlin, dated June* 12*th*, 1865.

"Berlin.

"My views respecting the way in which meteorological communications may be made available for practical use in storm-warnings are in general accordance with the methods followed in England; yet I acknowledge that I do not trust myself to announce daily probabilities, at least with the but limited communications which reach *me* telegraphically. My investigations in regard to storms have hitherto had relation to great atmospheric disturbances in autumn and winter, hardly at all to the storms of summer, in which the derangements of atmospheric equilibrium are much more local, and therefore the limits of the region overspread by the storm much narrower. This is particularly true of the storms of the Baltic. There, relative barometric minima occur, which seem to be cut off as it were towards the south. Probably the upper equatorial current first comes down in those high latitudes, breaking into the locally warm moist air, and occasioning a north-west wind at the south end of the Scandinavian mountain ranges, over the Kattegat and the lowlands of Denmark. Yet it is probable that these disturbances are less local than they may seem to the inhabitants of Western Europe, for they extend into the interior of Russia, and may become more intelligible when viewed in combination with telegraphically communicated data from Russia. All this must be studied if too hasty conclusions are to be avoided.

"We have introduced the English warning-signals into our Baltic ports. We leave to authorities at the ports who are conversant with the subject a discretionary power of showing warnings, in so far as they may be able to form a judgment from the telegrams which we send them of our observations here, and the general appearance of the sky, &c., viewed in connexion with the whole local character of the weather; but it is imperative on them to hoist a signal when an actual storm-warning is telegraphed from Berlin. I wished to introduce the system gradually. I consulted with Kupffer, who had similar views. His death is a new misfortune, following so shortly the loss of Admiral FitzRoy, to whom I owed great thanks for

much kindness. Also I had concerted with Kupffer and Plantamour for attending the Swiss Scientific Meeting at Geneva at the end of August, and inviting the heads of the different systems of observation to assemble there, and consult in common as to the best modes of treatment, communication, and publication. This must, no doubt, now stand over. As to the data to be communicated, it is no doubt right to give, as now, the height of the barometer at the moment the telegram is despatched; but I think it would be desirable to add a sign indicating whether the barometer is rising or falling. The cotemporaneous temperature is in many cases desirable, and thus I think that this communication might be so arranged that some scientific result could be based thereon. If the maximum and minimum of the preceding day had heretofore been telegraphed, we should have gained six or seven years' materials for enabling us to judge whether the day of the telegram was a relatively warm or cold one. The same hour has in the diurnal variation a very different meaning in different parts of the year; and in the summer months it is difficult to draw any definite conclusions from the temperatures at seven or eight o'clock. It seems to me, moreover, that in the present modes too little consideration is given to first laying down what it is desired to obtain. For England, for instance, the reduction of the barometer to the level of the sea is not difficult; but yet there are many land-stations in which one does not know whether this reduction has been made or not. Advances in meteorology are based on long-continued labours: we seem now to want to take it by storm; this may dazzle the public, but the results need control if they are to be recognized as really such.

"The idea that all storms are cyclones is indeed given up by most, and I have lately been taking some pains to contribute thereto. The introduction of the word 'cyclonoid' means nothing more than that for a given case it is wished to leave the matter undecided. It is a retrograde step.

"I have read with great interest the paper headed 'Forecasts and Cautions' kindly sent to me. It seems to me very suitable to the desired end. I cannot recognize any connexion with electrical currents; I cannot discern any proper bases for doing so.

"For meteorology itself, I should deem it extremely advantageous if, as you contemplate, the immediate data of observation in England were placed under a common guidance such as that of the Kew Observatory. The British Association does indeed represent in the freest and most independent manner scientific Great Britain as a whole. I do not certainly recognize what the British and Scottish Meteorological Societies have supplied in this direction; but an accordant mode of publication and treatment would still give quite other results. So, for example, in the monthly communications the notice of the barometric extremes of the month with the indispensable mention of the date of their occurrence is wanting. These are the very things by means of which it is possible to examine profitably the particular phenomena of a storm.

"The small pecuniary resources of our Meteorological Institute, which now includes ninety-seven stations, do not permit me to publish the daily means. I have therefore had to content myself with five-day means; but I think that by the consequent calculation of deviations I have brought some questions nearer to a solution. But I have to do this work by myself, and, overcharged as I am besides with official duties, I do not think I shall long be able to continue to master it. The resources which the British Association offers to all scientific undertakings in England will make it possible for you to establish in a thorough manner the constants of a climatology of England, and to investigate on this climatological basis the meteorology of England.

"I had long proposed to myself to write from my own point of view a pamphlet 'how to observe' in meteorology; but when one has, as I have constantly, to give lectures in the day, and hold examinations in the evening till nine o'clock, much that has been contemplated is left undone..... In regard to telegraphic communications, Admiral FitzRoy once said to me that reports from Eastern Europe were of little interest for England. This may be granted where it is question of the storms which assail the English and Irish coasts themselves. But the commerce of the Baltic is for the greater part in the hands of Englishmen, and I think that it would therefore be conducive to English interests also if the efforts were facilitated which are made by others to lessen the dangers to shipping in the Baltic. As it is precisely north-west storms which are the most dangerous in that sea, communications from England are wanted for this. Among the numerous telegraphic communications received here daily, there are none from England. Would it not be possible to arrange an exchange, if only for one or two stations? The communication might be made through Tönningen, by the cable, so as to avoid the German-Austrian Telegraph Company, which declines to afford a gratuitous passage for our messages.

"Harbour signal arrangements are now established at Memel, Pillau, Neufahrwasser, Stolpemünde, Rügenwalde, Colbergermünde, Swinemünde, Greifswald, Stralsund, and Barth."

June 15, 1865.

Major-General SABINE, President, in the Chair.

The Rev. W. R. Dawes, Mr. George Gore, Dr. George Harley, Mr. W. Huggins, Mr. Fleeming Jenkin, and Mr. W. K. Parker, were admitted into the Society.

The following communications were read:—

I. "Description of a Rigid Spectroscope, constructed to ascertain whether the Position of the known and well-defined Lines of a Spectrum is constant while the Coefficient of Terrestrial Gravity under which the Observations are taken is made to vary." By J. P. GASSIOT, V.P.R.S. Received May 18, 1865.

Shortly after my large spectroscope * had been removed to Kew Observatory, Mr. Stewart mentioned to me that he had had some conversation with Professor Tait of Edinburgh, as to the practicability of having a spectroscope constructed so as to preclude all errors of observations arising from a displacement of the prisms or the shifting of any of the fixed portions of the apparatus.

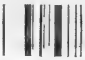
The particular object Mr. Tait and Mr. Stewart had in view, was the determining whether the positions of the known and well-defined lines of the spectrum are constant while the coefficient of terrestrial gravity, under which the observations are taken, is made to vary, Mr. Stewart considering that, provided an instrument could be constructed so rigid in all its parts as to preclude all possibility of error, the observations might be made in balloon ascents, varying from two to four miles.

I consulted with Mr. Browning as to the practicability of constructing the spectroscope. He considered such an instrument could be made, with sufficient rigidity in all its parts, to examine with great accuracy any given portion of the spectrum which might be selected, and for which the prisms would have to be adjusted and fixed. I communicated with Mr. Coxwell relative to the balloon ascents which would be required, and then determined on having the spectroscope constructed.

On testing the alteration in the position of the lines arising from change of temperature, it was soon ascertained that the difficulty of constructing a truly rigid spectroscope was far greater than had been anticipated.

By the description of the apparatus, it will be seen that the prisms are arranged so as to bring the D-lines into the centre of the field of view (fig. in margin), with a few of the fainter lines on each side; a perpendicular *fixed* line, and two cross *moveable* lines in the cobweb micrometer eyepiece, affording the means of measuring to $\frac{1}{10,000}$ of an inch, whatever alteration takes place in the position of the lines.

D



D-lines as seen with the Rigid Spectroscope.

The observations having been originally intended to be made in balloon ascents, the construction of the spectroscope had necessarily to be considered in reference to some portable and easily manageable form, and it was particularly desirable that its weight should be as low as possible. These conditions were obtained by constructing and mounting it in a T-shaped frame of gun-metal: in this manner the instrument was completed so as to weigh little more than 40 lbs.; but on carefully examining the readings day by day in Mr. Browning's workshop, the errors arising

* Proceedings of Royal Society, vol. xii. p. 536.

from changes in the temperature were ascertained to be so variable that no reliable result could have been obtained.

These preliminary observations were nevertheless so far valuable, for they proved that changes of temperature were taken up very slowly by the prisms, and that it would be consequently useless to employ the instrument in balloon ascents where rapid fluctuations of temperature would continually occur.

I then determined to attempt the construction of a rigid spectroscope with which observations might be made either on board a vessel or on land, in various latitudes; and as the question of the total weight of the apparatus became no longer of paramount importance, Mr. Browning decided on mounting the instrument in cast iron. The adjustments of the telescope being dispensed with, it was mounted in two cast-iron blocks, and fixed on a bed of slate; the prisms, with their adjustments, were attached to an iron plate, the plate being bolted to the same slate-bed. In this arrangement the observations still showed discrepancies, which were considered to arise from changes in the adjustments of the prisms, produced by alterations of temperature. Mr. Browning then removed all the adjustments of the prisms, and also the iron bed-plate, bolting the prisms on the bed of slate, and securing their correct position by filing and scraping. Full particulars of this arrangement will be found in the description of the apparatus.

The instrument has been carefully examined by Mr. Stewart, not only at Kew Observatory but also from time to time during the progress of its construction, as well as after it was completed in Mr. Browning's workshops; and it may now be considered that, with ordinary care during its transit from place to place, any observations made with it can be depended on as far as the mechanical arrangement is concerned.

I am indebted to Mr. Browning for the description of the apparatus, with the notes of the readings as they were made by himself.

The optical arrangement is as follows:—In order to obtain great refractive power in a moderate compass, the prisms were arranged as in Plate VI. fig. 1. P and P′ represent two prisms of heavy flint glass, having sides $2\frac{1}{2}$ inches high, and 3 inches long. These prisms have refracting angles of 45°. They are arranged at the minimum angle of deviation for Fraunhofer's line D. R represents a prism of similar material and dimensions, but with a refracting angle of 22° 30′, that is half P and P′.

The dense flint glass of which these prisms are composed was made by Messrs. Chance Brothers and Co. It has a specific gravity of 3·9. Its mean refractive index is 1·665, and its dispersive power 0·0752. The prism R has the side further from P and P′ silvered. The nearest side is placed at the same minimum angle of deviation from P as P is from P′.

D and D′ represent a compound prism, formed by cementing a very small diagonal prism D′ on to a large diagonal prism D with a transparent cement, in such a manner that two of the plane surfaces are parallel. They

are both made of hard white optic crown glass, cut from the same block. O is an achromatic object-glass of 2·3 inches aperture, and 3 feet focal length. M is a cobweb micrometer eyepiece, having one fixed vertical web, and two which are crossed, moving together; also a fine rack in the field of view, which serves to register whole turns of the micrometer head (fig. 4). S is a pair of knife-edges. The action is as follows: when any source of light is brought in front of the knife-edges S, some of the rays emitted pass through them, and unchanged through the double diagonal prism D, D′, as shown on a large scale in diagram 2. As the object-glass O is placed at its focal distance from the knife-edges, the rays in passing through it are rendered parallel; on entering and emerging from P′, P these rays suffer refraction, and also, if the light be not homogeneous, dispersion. The same effects are produced as the rays enter the first surface of R, and again emerge from it, after being reflected from the further side, which, as has been previously mentioned, is silvered. They now retrace their way through the prisms P and P′, the refraction and dispersion being doubled in this return passage. In this manner a result is obtained equal to that which would be produced by five prisms, if employed in the ordinary manner.

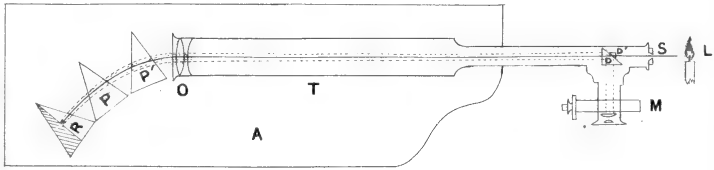
Repassing through O, this compound lens, which before acted as a collimator, now acts as the object-glass of the telescope T. The cone of rays produced by this lens falls on the prism D D′, figs. 1 & 2, and is reflected from the diagonal side, a loss of light determined by the size of the small prism D being experienced; but as this prism need be but little more than the length and width of the slit formed by the knife-edges, the loss may, practically, be considered unimportant.

In figs. 1 & 2 the continuous line represents the rays of light in their first passage through the prisms, and the dotted lines the same rays returning through the instrument. The image of the slit is viewed, and any change in its position observed, by means of the micrometer eyepiece M. Owing to the power of the instrument, only a very small portion of the spectrum can be seen at once in the field of view. The reflecting prism R is, however, provided with a tangent screw motion, which affords the means of bringing any portion of the spectrum into the field of view that it may be desired to examine.

Although having to contend with several disadvantages on the score of reflexions not made use of in spectroscopes of the ordinary construction, and which of course cause loss of light and tend to deteriorate the definition, yet it will, I think, be admitted that the performance of the instrument is satisfactory.

Mr. W. Huggins has seen two bright lines between the D-lines produced by the flame of a common spirit lamp; and several persons have seen on different occasions from five to seven lines between the D-lines in the solar spectrum. This is equal to the performance of my large spectroscope, with which the solar spectrum is now being mapped at Kew Ob-

Fig 1.



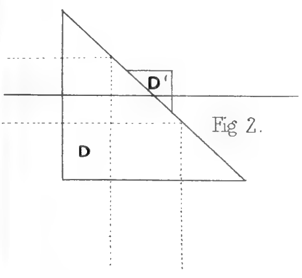


Fig 2.

Fig 4.

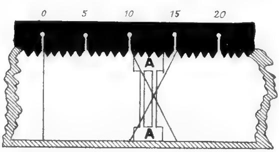
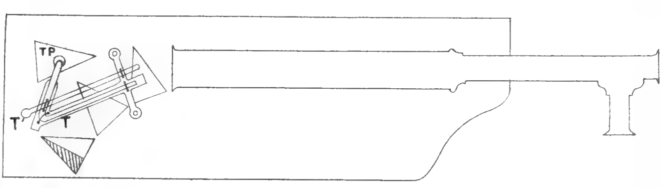


Fig 3.



J. Basire lith.

servatory, although that instrument has nine prisms. The prisms of that spectroscope are, however, but little more than half the size of those in the rigid instrument now described. It is to the large size of the prisms, and the greater aperture and focal length of the object-glass, that the superior performance of this instrument must be attributed.

The temperature of the air and of the prisms is thus observed:—A third prism (T P, fig. 3), exactly similar to P and P′, is mounted on the slate block. This prism has a hole about $\frac{1}{2}$ an inch diameter and $1\frac{1}{2}$ inch deep drilled in it vertically. A thermometer (Plate VI. T, fig. 3) with a fine cylindrical bulb is inserted in this hole, the intervening space being packed tight with copper filings. The upper part is covered with a layer of fused shell-lac. That the half prism may be about the same temperature as the whole prisms, another half is cemented to it. To avoid confusion, this half is not represented in the diagrams. The thermometer, after leaving the prism, is bent at right angles, and is carried across the top of the prisms on a light metal frame. Another thermometer (T′, fig. 3), whose bulb is in the air, its object being to denote the temperature of the air around the prisms, runs parallel with that just described. Both the prisms and thermometers are enclosed under a metal cover, for the purpose of equalizing the temperature and protecting them from injury. This cover has a long slip of stout plate-glass let into the upper part of it, through which the thermometers can be seen, and their readings observed. The micrometer eyepiece and the cell containing the object-glass are each mounted on distinct iron blocks. The body of the telescope which fills up the intervening space is mounted on two separate iron blocks not connected with those just mentioned. The tube which forms the body overlaps at one end the tube of the eyepiece, and at the other the mounting of the object-glass, but without being in contact with either. By this means the change of length in the body-tube, produced by change of temperature, by far the most considerable change we have to contend with, is prevented from exerting any influence on the indications given by the instrument.

The homogeneous light of the sodium-flame is employed, and the micrometer wires are lighted up by the contrivance shown in fig. 4. A portion of the cap on which the knife-edges are fitted is cut away, and the light which is thus admitted enables the wires, and the rack that serves to register whole turns of the micrometer screw, to be seen distinctly *. A whole turn of the micrometer screw values $\frac{1}{100}$ of an inch, so that the first reading in Table I. might have been written 0·0552. In making remarks on differences in the readings, they will be expressed in this manner, the reading of the line D decreasing with the rise in the temperature of the prisms.

The object in taking the readings of the D-line, which are appended, was

* In fig. 4 the bright lines of sodium, as seen in the rigid spectroscope, are represented; the spaces A, A are cut away, allowing the light from the sodium-flame to enter and illuminate the field enough to render the cobwebs visible.

to endeavour to determine the temperature corrections which it will be necessary to apply to the results obtained by the instrument, and also to find if the line resumed its position exactly after the instrument had been subjected to considerable changes of temperature, or carried from place to place. From Tables III. and IV. it may be noted that the readings have a constant downward tendency. To endeavour to account for this retrogression, I can only venture to hazard the suggestion that the index of refraction of the glass of which the prisms are made may be slightly varying from some change due to annealing not having yet been entirely completed.

Although showing generally a downward tendency, the decline is not quite uniform, and there are some apparent discrepancies in the readings; these may be errors of observation, arising principally from differences of intensity in the source of illumination. Such differences tend to alter the width of the bright line observed; and as one of its edges is taken for the point of measurement, the position of the line is apparently changed. Mr. Stewart is, however, of opinion that these variations are so slight that they will not be likely to interfere with the instrument being used for the purpose for which it was designed.

Table I.

Readings of one of the D-lines taken with the Rigid Spectroscope at the Minories.

Date.	Temp. Air.	Temp. Prism.	Micrometer.	Difference in divisions.
February 1.	57°·5	52°·5	5·52	1·20
„ 1.	88·0	82·5	6·72	
„ 2.	90·0	82·5	6·55	1·27
„ 3.	52·0	52·5	5·28	
„ 6.	56·0	52·5	4·98	1·17
„ 6.	90·5	82·5	6·15	
„ 7.	53·0	53·0	5·05	1·18
„ 7.	95·5	82·5	6·23	
„ 9.	54·0	52·5	4·85	1·25
„ 9.	84·0	82·5	6·10	
„ 11.	57·5	52·5	4·85	

Results from 6th to 11th:—

Variation in temperature correction for a change of temperature of 30° .... 0·0007 inch.

Separation of D-lines .... 0·004 inch.

TABLE II.

Date.		Temp. Air.	Temp. Prism.	Readings.		Difference for 20°.
February 16.	....	48°·5	42°·5	4·60		
,, ,,	....	58·0	52·5	5·09	=	0·96
,, ,,	....	69·5	62·5	5·56		
,, 17.	....	50·0	42·5	4·		
,, ,,	....	57·5	52·5	5·08	=	0·92
,, ,,	....	67·5	62·5	5·54		
,, 18.	....	48·0	42·5	4·60		

Variation of temperature correction for a change of temperature of 20° .... 0·0004 inch.

TABLE III.

Date.		Temp. Air.	Temp. Prism.	Readings.
February 20.	....	56°·0	52°·5	4·98
,, 21.	....	56·0	,, ,,	4·88
,, 22.	....	58·0	,, ,,	4·90
,, 23.	....	53·0	,, ,,	4·89
,, 24.	....	53·5	,, ,,	4·92
,, 27.	....	53·0	,, ,,	4·88
,, 28.	....	52·5	,, ,,	4·94
March 1.	....	56·0	,, ,,	4·96
,, 2.	....	56·5	,, ,,	4·93
,, 7.	....	56.0	,, ,,	4·84
,, 8.	....	56·0	,, ,,	4·84
,, 9.	....	58·0	,, ,,	4·82

Decrease of zero, 0·0016 of an inch.

Having thus far satisfied ourselves by observing under varied temperatures, and also that the removal of the instrument to different parts of Mr. Browning's premises did not affect the readings on the 22nd of March, the apparatus was removed in a cart to Kew Observatory, and placed in one of the rooms for observing, when the following readings were made :—

Date.		Temp. Air.	Temp. Prisms.	Reading.
March 23,	....	45°·0	42°·2	4·57

Mean of readings taken in the Minories, at a temperature of 42°·5, 4·60 ; change of zero during the transit, 0·0003 of an inch.

Observations were subsequently continued under the direction of Mr. Stewart.

TABLE IV.

Readings taken by Mr. Beckley at Kew Observatory.

Date.		Temp. Air.	Temp. Prism.	Micrometer reading.
		°	°	
April 3.	....	57·5	56·8	5·73
„ 28.	....	57·2	56·8	5·53
„ 3.	....	55·1	54·3	5·66
„ 6.	....	56·0	54·5	5·41
„ 9.	....	55·0	58·4	5·44
May 3.	....	53·6	58·4	5·47

Decrease of zero in the month, ·0026.

On the 5th of May the spectroscope was removed to the rooms of the Royal Society, Burlington House, where it still remains.

The result of observing under a varied temperature of 40° Fahr., the carrying of the instrument from the Minories to Kew Observatory, and subsequently to the Royal Society, without affecting the readings, may be taken as evidence that with ordinary care the spectroscope can now be used with reliance as to the rigidity of its construction, thus fulfilling the conditions which are indispensable for obtaining correct observations.

It will be observed that it was my intention to have made arrangements with Mr. Coxwell for the observations being made with his balloon, but the weight of the entire apparatus (approaching two hundred weight), and still more the difficulty of obtaining a uniform temperature throughout the prisms, renders observing in this manner very difficult, if not impracticable; I therefore suggested to Mr. Stewart that, if the observations were made in different latitudes, the object sought would be obtained in a more satisfactory manner.

The best, and probably the most satisfactory mode of observing, would be to obtain the sanction of the Admiralty to allow the spectroscope to be placed on board one of Her Majesty's vessels about visiting various latitudes; continued observations could then be made, and the result thereof from time to time forwarded to Kew Observatory.

Mr. Stewart writes me, that to this time it has been assumed, without proof, that the change of the coefficient of terrestrial gravity does not in itself alter any other coefficient of a body; and if a reason is asked, none can be given, since gravity is a force of the nature of which men of science are confessedly ignorant, and that it would therefore be very desirable that experiments should be undertaken with the view of setting this matter at rest.

It is to determine this, as far as the index of refraction is concerned, that the spectroscope I have described has been constructed, and the assistance of the President and Council of the Royal Society will be asked, in order that the observations may be made with this apparatus by some trustworthy observer, on board any of Her Majesty's ships, from one point to another of the earth's surface.

II. "A Description of some Fossil Plants, showing structure, found in the Lower Coal-seams of Lancashire and Yorkshire." By E. W. Binney, F.R.S. Received May 12, 1865.

(Abstract.)

The author stated that, although great attention has been devoted to the collection of the fossil remains of plants with which our coal-fields abound, the specimens are generally in very fragmentary and distorted conditions as they occur imbedded in the rocks in which they are entombed; but when they have been removed, cut into shape, and trimmed, and are seen in cabinets, they are in a far worse condition. This is as to their external forms and characters. When we come to examine their internal structure, and ascertain their true nature, we find still greater difficulties, from the rarity of specimens displaying both the external form and the internal structure of the original plant. It is often very difficult to decide which is the outside, different parts of the stem dividing and exposing varied surfaces which have been described as distinct genera of plants.

The specimens described were collected by the author himself, and taken out of the seams of coal, just as they occurred in the matrix in which they were found imbedded, by his own hands. This has enabled him to speak with certainty as to the condition and locality in which they were met with.

By the ingenuity of the late Mr. Nicol of Edinburgh, we were furnished with a beautiful method of slicing specimens of fossil-wood so as to examine their internal structure. The late Mr. Witham, assisted by Mr. Nicol, first applied this successfully, and his work on the internal structure of fossil vegetables was published in 1833. In describing his specimens, he notices one which he designated *Anabathra pulcherrima*. This did not do much more than afford evidence of the internal vascular cylinder arranged in radiating series, somewhat similar to that described by Messrs. Lindley and Hutton as occurring in *Stigmaria ficoides*, in the third volume of the 'Fossil Flora.'

In 1839 M. Adolphe Brongniart published his truly valuable memoir, "Observations sur la structure intérieure du *Sigillaria elegans* comparée à celle des *Lepidodendron* et des *Stigmaria* et à celle des végétaux vivants," in the Archives du Muséum d'Histoire Naturelle. His specimen of *Sigillaria elegans* was in very perfect preservation, and showed its external characters and internal structure in every portion except the pith and a broad part of the plant intervening betwixt the internal and external radiating cylinders. Up to this time nothing had been seen at all to be compared to M. Brongniart's specimen, and no person could have been better selected to describe and illustrate it. His memoir will always be considered as one of the most valuable ever contributed on the fossil flora of the Carboniferous period.

In 1849, August Joseph Corda published his 'Beiträge zur Flora der Vorwelt,' a work of great labour and research. Amongst his numerous specimens, he describes and illustrates one of *Diploxylon cycadeoideum*, which, although not to be compared to M. Brongniart's specimen, still affords us valuable information, confirming some of that author's views rather than affording much more original information. All these last three specimens M. Brongniart, in his 'Tableau de végétaux fossiles considérées sous le point de vue de leur classification botanique et de leur distribution géologique,' published in 1847, classes as *Dicotyledones gymnospermes* under the family of *Sigillarées*; amongst other plants his *Sigillaria elegans*, Mr. Witham's *Anabathra*, and Corda's *Diploxylon*.

In 1862 the author published, in the 'Quarterly Journal of the Geological Society' of that year, an account of specimens which confirmed the views of the three learned authors above named as to *Sigillaria* and *Diploxylon* being allied plants; but showed that their supposed pith or central axis was not composed of cellular tissue, but of different sized vessels arranged without order, having their sides barred by transverse striæ like the internal vascular cylinders of *Sigillaria* and *Lepidodendron*. These specimens were in very perfect preservation, and showed the external as well as the internal characters of the plants.

All the above specimens were of comparatively small size, with the exception of that described by Mr. Corda, which, although it showed the external characters in a decorticated state, did not exhibit any outward resemblance to a plant allied to *Sigillaria* with large ribs and deep furrows so commonly met with in our coal-fields, but rather to plants allied to *Sigillaria elegans* and *Lepidodendron*.

In the present communication the author has described some specimens of larger size than those previously alluded to, and endeavoured to show that the *Sigillaria vascularis* with rhomboidal scars gradually passes as it grows older into ribbed and furrowed *Sigillaria*, and that this singular plant not only possesses two woody cylinders arranged in radiating series, an internal and an external one divided by a zone of cellular tissue, both increasing on their outsides at the same time, but likewise has a central axis composed of hexagonal vessels, arranged without order, having all their sides marked with transverse striæ. Evidence is also adduced to show that *Sigillaria* dichotomizes in its branches something like *Lepidodendron*, and that, like the latter plant, a *Lepidostrobus* is its fructification. The outer cylinder in large *Sigillaria* is composed of thick-walled quadrangular tubes or utricles arranged in radiating series, and exhibiting every appearance of having been as hard-wooded a tree as *Pinites*, but as yet no disks or striæ have been observed on the walls of the tubes. *Stigmaria* is now so generally considered to be the root of *Sigillaria*, that it is scarcely necessary to bring any further proof of this proposition; but specimens are described which prove by similarity of structure that the former is the root of the latter.

The chief specimens described in the memoir are eight in number, and were found in the lower divisions of the Lancashire and Yorkshire coal-measures imbedded in calcareous nodules occurring in seams of coal.

No. 1, *Diploxylon cycadoideum,* was from the first-named district, and the same locality as the *Trigonocarpon,* described by Dr. J. D. Hooker, F.R.S., and the author, in a memoir on the structure of certain limestone nodules inclosed in seams of bituminous coal, with a description of some *Trigonocarpons* contained therein*, and the other seven (*Sigillaria vascularis*) were from the same seam of coal in the lower coal-measures in which the specimens described in a paper entitled "On some Fossil Plants showing structure from the Lower Coal-measures of Lancashire"†, were met with, but from a different locality in Yorkshire.

III. "On Symbolical Expansions." By W. H. L. Russell, Esq., A.B. Communicated by Prof. Stokes, Sec. R.S. Received May 13, 1865.

Among the papers on symbolical algebra by the lamented Professor Boole, there is one on the Theory of Development, published in the fourth volume of the 'Cambridge Mathematical Journal.' The expansion of $f\left(x+\frac{d}{dx}\right)$ is there given in a very elegant form. I am desirous to terminate my own investigations on the Calculus of Symbols by pointing out the connexion of the binomial theorems given in my first paper on this subject with the expansions due to Professor Boole, and propose with that view to expand $f\left(x+x\frac{d}{dx}\right)$ in terms of $\frac{d}{dx}$, which will be sufficient to indicate the general method. When the term of the expansion which does not contain $\frac{d}{dx}$ is known, the other terms are easily found by a method given by Professor Boole in the paper I have just mentioned. The main object of the present paper, therefore, will be to ascertain that part of the expansion of $f\left(x+x\frac{d}{dx}\right)$ which does not contain $\frac{d}{dx}$.

Putting, as usual, ρ for (x) and π for $x\frac{d}{dx}$, the expression becomes $f(\rho+\pi)$. Our first object must be to ascertain that part of the expansion of $(\rho+\pi)^n$ which is independent of (π), from whence we may easily deduce the corresponding portion of $f(\rho+\pi)$. Now by a former paper the part of $(\rho+\pi)^n$, independent of π, will be

$$\rho^n+\Sigma n\,.\,\rho^{n-1}+\Sigma(n-1)\Sigma n\rho^{\ -2}+\Sigma(n-2)\Sigma(n-1)\Sigma n\rho^{n-3}$$
$$+\ \&c.\ +\Sigma(n-r+1)\Sigma(n-r+2)\ \ldots\ldots\ \Sigma n\rho^{n-r}+\ \ldots\ldots$$

* Philosophical Transactions, 1855, p. 149.

† Quarterly Journal of the Geological Society of London for May 1862.

And we must first endeavour to find a suitable expression for

$$\Sigma(n-r+1)\Sigma(n-r+2)\Sigma(n-r+3)\,\ldots\ldots\,\Sigma n.$$

With this purpose let us assume

$$\begin{aligned}&\Sigma(n-r+1)\Sigma(n-r+2)\,\ldots\ldots\,\Sigma n=\\&A_r^{(1)}(n-2r+1)(n-2r+2)\,\ldots\ldots\,n\\&+A_r^{(2)}(n-2r+2)(n-2r+3)\,\ldots\ldots\,n\\&+A_r^{(3)}(n-2r+3)(n-2r+4)\,\ldots\ldots\,n+\&c.\end{aligned}$$

Whence

$$\begin{aligned}&\Sigma(n-r)\Sigma(n-r+1)\Sigma(n-r+2)\,\ldots\ldots\,\Sigma n=\\&\frac{A_r^{(1)}}{2r+2}(n-2r-1)(n-2r)(n-2r+1)\,\ldots\ldots\,n\\&+\frac{rA_r^{(1)}}{2r+1}(n-2r)(n-2r+1)\,\ldots\ldots\,n\\&+\frac{A_r^{(2)}}{2r+1}(n-2r)(n-2r+1)\,\ldots\ldots\,n\\&+\frac{(r-1)A_r^{(2)}}{2r}(n-2r+1)(n-2r+2)\,\ldots\ldots\,n\\&+\frac{A_r^{(3)}}{2r}(n-2r+1)(n-2r+2)\,\ldots\ldots\,n\\&+\frac{(r-2)A_r^{(3)}}{2r-1}(n-2r+2)(n-2r+3)\,\ldots\ldots\,n\\&+\ \&c.\\&=A_{r+1}^{(1)}(n-2r-1)(n-2r)\,\ldots\ldots\,n\\&+A_{r+1}^{(2)}(n-2r)(n-2r+1)\,\ldots\ldots\,n\\&+A_{r+1}^{(3)}(n-2r+1)(n-2r+2)\,\ldots\ldots\,n+\ \&c.\end{aligned}$$

Hence

$$A_{r+1}^{(1)}=\frac{A_r^{(1)}}{2r+2},\quad A_{r+1}^{(2)}=\frac{A_r^{(2)}}{2r+1}+\frac{rA_r^{(3)}}{2r+1}$$

$$A_{r+1}^{(3)}=\frac{A_r^{(3)}}{2r}+\frac{(r-1)A_r^2}{2r}\,\ldots\,\&c.$$

This will give us

$$A_r^{(1)}=\frac{1}{2r(2r-2)(2r-4)\,\ldots\ldots\,2}$$

$$A_r^{(2)}=\frac{1}{(2r-1)(2r-3)\,\ldots\ldots\,1}\,\Sigma\frac{r(2r-1)(2r-3)\,\ldots\ldots\,1}{2r(2r-2)\,\ldots\ldots\,2}$$

&c. = &c.

$$A_r^{(3)}=\frac{1}{(2r-2)(2r-4)\,\ldots\ldots\,2}\,\Sigma\frac{(r-1)(2r-2)(2r-4)\,\ldots\ldots\,2}{(2r-1)(2r-3)\,\ldots\ldots\,1}$$

$$\Sigma\frac{r(2r-1)(2r-3)\,\ldots\ldots\,1}{2r(2r-2)(2r-4)\,\ldots\ldots\,2}.$$

Hence we have generally, using Π as a symbol for a continued product,

$$A_r^{(m)}=\frac{1}{\Pi(2r-m+1)}\Sigma(r-m+2)\frac{\Pi(2r-m+1)}{\Pi(2r-m+2)}$$

$$\Sigma(r-m+3)\frac{\Pi(2r-m+2)}{\Pi(2r-m+3)}\Sigma\ldots\Sigma r\frac{\Pi(2r-1)}{\Pi 2r};$$

whence the portion of $(\rho+\pi)^n$ which does not contain (π) may be written

$$\begin{aligned}&\rho^n+A_1^{(1)}n(n-1)\rho^{n-1}\\&+\{A_2^{(1)}(n-3)(n-2)(n-1)n+A_2^{(1)}(n-2)(n-1)n\}\rho^{n-2}\\&+\{A_3^{(1)}(n-5)(n-4)(n-3)(n-2)(n-1)n+\\&\{A_3^{(2)}(n-4)(n-3)(n-2)(n-1)n+A_3^{(3)}(n-3)(n-2)(n-1)n\}\rho^{n-3}\\&+\ \&c.\\&=\rho^n+A_1^{(1)}\rho\frac{d^2}{d\rho^2}\rho^n+\left\{A_2^{(1)}\rho^2\frac{d^4}{d\rho^4}+A_2^{(2)}\rho\frac{d^3}{d\rho^3}\right\}\rho^n\\&+\left\{A_3^{(1)}\rho^3\frac{d^6}{d\rho^6}+A_3^{(2)}\rho^2\frac{d^5}{d\rho^5}+A_3^{(3)}\rho\frac{d^4}{d\rho^4}\right\}\rho^n+\ldots,\end{aligned}$$

the general term being $A_r^{(m)}\rho^{r-m+1}\dfrac{d^{2r-m+1}}{d\rho^{2r-m+1}}\rho^n$;

whence the part of the expansion of $f(\rho+\pi)$, which does not contain π, is

$$f(\rho)+A_1^{(1)}\rho\frac{d^2}{d\rho^2}f(\rho)+\left\{A_2^{(1)}\rho^2\frac{d^4f\rho}{d\rho^4}+A_2^{(2)}\rho\frac{d^3f(\rho)}{d\rho^3}\right\}$$
$$+\ \&c.$$

If, then, we put

$$f\left(x+x\frac{d}{dx}\right)=f_0(x)+f_1(x)\frac{d}{dx}+f_2(x)\frac{d^2}{dx^2}+\ldots,$$

we have

$$f_0(x)=f(x)+A_1^{(1)}x\frac{d^2f(x)}{dx^2}+\left\{A_2^{(1)}x^2\frac{d^4f(x)}{dx^4}+A_2^{(2)}x\frac{d^3f(x)}{dx^3}\right\}$$

$$+\left\{A_3^{(1)}x^3\frac{d^6f(x)}{dx^6}+A_3^{(2)}x^2\frac{d^5f(x)}{dx^5}+A_3^{(3)}x\frac{d^4f(x)}{dx^4}\right\}$$

$$+\ \&c.,$$

the general term being

$$A_r^{(m)}x^{r-m+1}\frac{d^{2r-m+1}f(x)}{dx^{2r-m+1}},$$

where $A_r^{(m)}$ has the value given above; and $f_1(x)$, $f_2(x)$, $f_3(x)$, &c. are given by the following formula, which, as I have before said, can be immediately

deduced from one in the paper of Professor Boole on the Theory of Development:

$$f_{n+1}(x)=\frac{x}{n+1}f'_n(x)-\frac{n}{n+1}f_n(x).$$

The method of the present paper is of course of far more general application; but I have said enough in it to explain the principle on which such expansions must be conducted.

IV. "On the Summation of Series." By W. H. L. Russell, Esq., A.B. Communicated by Professor Stokes, Sec. R.S. Received May 13, 1865.

In a Memoir published in the Philosophical Transactions for the year 1855, I applied the Theory of Definite Integrals to the summation of many intricate series. I have thought my researches on this subject might well be terminated by the following paper, in which I have pointed out methods for the summation of series of a far more complicated nature.

I commence with some remarks intended to give clear conceptions of the general method of calculation.

In any series,

$$u_0+\alpha u_1+\alpha^2 u_2+\alpha^3 u_3+\&c.+\alpha^x u_x+\&c.$$

Where α is less than unity, it is evident that we can sum the series by a definite integral when $u_x=\int du\, U_1\, U^x$, U_1 and U being functions of u, and the integral being taken between certain assigned limits. For it is manifest that the quantity under the integral sign then becomes a geometrical progression.

Again, for a similar reason we can express by a definite integral the sum of the series

$$u_0v_0w_0\ldots+\alpha u_1v_1w_1\ldots+\alpha^2u_2v_2w_2\ldots+\&c.$$
$$+\alpha^x u_xv_xw_x\ldots+\&c.,$$

where

$$u_x=\int du\, U_1U^x,\quad v_x=\int dv V_1V^x,$$

$$w_x=\int dw W_1W^x,\ \&c.$$

Lastly, we can sum the series

$$u_0v_0w_0\ldots+\alpha\, u_1v_1w_1\ldots+\alpha^2u_2v_2w_2\ldots+\&c.$$
$$+\alpha^x u_xv_xw_x\ldots+\&c.$$

by a definite integral when

$$u_x = \int du U_1 U^x + \int du' U'_1 U'^x + \int du'' U''_1 U''^x + \ldots$$

$$v_x = \int dv V_1 V^x + \int dv' V'_1 V'^x + \int dv'' V''_1 V''^x + \ldots$$

$$w_x = \int dw W_1 W^x + \int dw' W'_1 W'^x + \int dw'' W''_1 W''^x + \ldots$$

&c. = &c.,

the number of each set of quantities u_1, u', &c., v_1, v', &c., w_1, w', &c. being of course finite.

I shall now consider the series

$$\phi(0)^{\psi(0)} + \alpha\phi(1)^{\psi(1)} + \alpha^2\phi(2)^{\psi(2)} + \text{\&c.}$$
$$+ \alpha^x\phi(x)^{\psi(x)} + \text{\&c.},$$

where $\phi(x)$ and $\psi(x)$ are rational functions of (x).

Let

$$\phi(x) = \frac{(\alpha+\beta x)(\alpha_1+\beta_1 x)(\alpha_2+\beta_2 x)\ldots}{(a+bx)(a_1+b_1x)(a_2+b_2x)\ldots},$$

$$\psi(x) = \frac{1}{m+nx} + \frac{1}{m_1+n_1x} + \frac{1}{m_2+n_2x} + \text{\&c.}$$

Hence, by what has been said, the problem is reduced to finding a definite integral of the form $\int du\, U_1 U_x$ equivalent to $\left\{\frac{\alpha+\beta x}{a+bx}\right\}^{\frac{1}{m+nx}}$.

Now

$$\left\{\frac{\alpha+\beta x}{a+bx}\right\}^{\frac{1}{m+nx}} = \frac{1}{\Gamma\left\{\frac{1}{m+nx}\right\}} \int_0^1 du\,.\, u^{\frac{a+bx}{\alpha+\beta x}-1} \left(\log_e \frac{1}{u}\right)^{\frac{1}{m+nx}-1}$$

$$= \frac{1}{\pi}\sin\frac{\pi}{m+nx}\,\Gamma\left\{\frac{m+nx-1}{m+nx}\right\} \int_0^1 du\,.\, u^{\frac{a+bx}{\alpha+\beta x}-1} \left(\log_e \frac{1}{u}\right)^{\frac{1}{m+nx}-1}$$

$$\sin\frac{\pi}{m+nx} = \frac{1}{\sqrt{2}}\cos\left\{\frac{\pi}{4} - \frac{\pi}{m+nx}\right\} - \frac{1}{\sqrt{2}}\sin\left\{\frac{\pi}{4} - \frac{\pi}{m+nx}\right\}$$

$$= \frac{\sqrt{2}}{\pi}\int_0^1 ds \log_e^{-\frac{1}{2}}\frac{1}{s}\,.\, s^{\frac{1}{m+nx}-1} \int_0^\infty dz \cos(m+nx)z^2 \cos 2\sqrt{\pi}\,.\,z$$

$$- \frac{\sqrt{2}}{\pi}\int_0^1 ds \log_e^{-\frac{1}{2}}\frac{1}{s}\,.\, s^{\frac{1}{m+nx}-1} \int_0^\infty dz \sin(m+nx)z^2 \cos 2\sqrt{\pi}\,.\,z$$

$$\Gamma\left\{\frac{m+nx-1}{m+nx}\right\}=\int_0^\infty \epsilon^{-v}\,dv\,\epsilon^{-\frac{1}{m+nx}}$$

$$\int_0^1 du\,.\,u^{\frac{a+bx}{\alpha+\beta x}-1}\left(\log_\epsilon\frac{1}{u}\right)^{\frac{1}{m+nx}-1}$$

$$=\int_{\frac{1}{\epsilon}}^1 du\,.\,\epsilon^{\log_\epsilon u\left(\frac{a+bx}{\alpha+\beta x}-1\right)}\epsilon^{\log_\epsilon\log_\epsilon\frac{1}{u}\left(\frac{1}{m+nx}-1\right)}$$

(where $\log_\epsilon \log_\epsilon \frac{1}{u}$ is negative),

$$+\int_0^{\frac{1}{\epsilon}} du\;\epsilon^{\log_\epsilon u\left(\frac{a+bx}{\alpha+\beta x}-1\right)}\epsilon^{\log_\epsilon\log_\epsilon\frac{1}{u}\left(\frac{1}{m+nx}-1\right)}$$

(where $\log_\epsilon \log_\epsilon \frac{1}{u}$ is positive).

By means of these transformations the series is reduced to forms considered in my previous investigations; for the general term of the transformed series included under the signs of definite integration is of the form

$$P\alpha^x\,\epsilon^{\frac{\phi}{m+nx}}\{\cos h(m+nx)-\sin k(m+nx)\},$$

a form which I have discussed in the memoir in the Philosophical Transactions mentioned at the beginning of this paper.

Next let us investigate the series

$$\sqrt[n]{\psi(0)+\sqrt[m]{\chi(0)}}+\alpha\sqrt[n]{\psi(1)+\sqrt[m]{\chi(1)}}+\ldots+\alpha^x\sqrt[n]{\psi(x)+\sqrt{\chi(x)}}+\ldots,$$

where $\psi(x)$ and $\chi(x)$ are identical functions of (x).

We transform as follows:—

$$\sqrt[n]{\psi(x)+\sqrt[m]{\chi(x)}}=\frac{1}{\Gamma\left(\frac{1}{n}\right)}\int_0^1 du\,u^{\frac{1}{\psi(x)+\sqrt[m]{\chi(x)}}-1}\left(\log_\epsilon\frac{1}{u}\right)^{n-1}$$

$$\epsilon^{\frac{\log_\epsilon u}{\psi(x)+\sqrt[m]{\chi(x)}}}=\frac{\sqrt{\psi(x)+\sqrt[m]{\chi(x)}}}{2\sqrt{\pi}\log_\epsilon^{\frac{1}{2}}\frac{1}{u}}\int_{-\infty}^{\infty}d\rho\;\epsilon^{-\frac{\psi(x)+\sqrt[m]{\chi(x)}}{4\log_\epsilon\frac{1}{u}}\rho^2}\cos\rho,$$

remembering that $\log_\epsilon u$ is negative,

$$\sqrt{\psi(x)+\sqrt[m]{\chi(x)}}=\frac{\psi(x)+\sqrt[m]{\chi(x)}}{\sqrt{\pi}}\int_{-\infty}^{\infty}\epsilon^{-v^2(\psi(x)+\sqrt[m]{\chi(x)})}dv,$$

$$\epsilon^{-\left(v^2+\frac{\rho^2}{4\log_\epsilon\frac{1}{u}}\right)\sqrt[m]{\chi(x)}}=\frac{1}{\pi}\int_0^\pi\frac{(1-\sqrt[m]{\chi(x)}^2)F(\theta)d\theta}{1-2\sqrt[m]{\chi(x)}\cos\theta+\sqrt[m]{\chi(x)}^2},$$

where

$$F(\theta)=\epsilon^{-\left(v^2+\frac{\rho^2}{4\log_\epsilon\frac{1}{u}}\right)\cos\theta}\cos\left\{\left(v^2+\frac{\rho^2}{4\log_\epsilon\frac{1}{u}}\right)\sin\theta\right\},$$

and $\sqrt[m]{\chi(x)}$ is supposed less than unity.

Hence also

$$\epsilon^{-\left(v^2+\frac{\rho^2}{4\log_\epsilon\frac{1}{u}}\right)\sqrt[m]{\chi(x)}}$$

$$=\frac{1}{\pi}\int_0^\pi\frac{(1-\sqrt[m]{\chi(x)^2})F_1(\chi(x),\epsilon^{i\theta})F_1(\chi(x),\epsilon^{-i\theta})F(\theta)d\theta}{1-2\chi(x)\cos m\,\theta+\chi(x)^2},$$

where we render the denominator rational by multiplication, and suppose

$$F_1(\chi(x),\epsilon^{i\theta})=\chi(x)^{\frac{m-1}{m}}+\epsilon^{i\theta}\chi(x)^{\frac{m-2}{m}}+\epsilon^{2i\theta}\chi(x)^{\frac{m-3}{m}}+\ldots,$$

$$F_1(\chi(x),\epsilon^{-i\theta})=\chi(x)^{\frac{m-1}{m}}+\epsilon^{-i\theta}\chi(x)^{\frac{m-2}{m}}+\epsilon^{-2i\theta}\chi(x)^{\frac{m-3}{m}}+\ldots,$$

$$\frac{1}{1-2\chi(x)\cos m\theta+\chi(x)^2}=\frac{\chi_1(x)^2}{(\chi_1(x)-\cos m\theta)^2+\sin^2 m\theta},$$

where

$$\chi_1(x)=\frac{1}{\chi(x)}.$$

$\chi(x)$ is less than unity, hence $\chi_1(x)$ is greater than unity, and therefore $\chi_1(x)-\cos m\,\theta$ is always positive; hence

$$\frac{1}{1-2\chi(x)\cos m\,\theta+\chi(x)^2}=\frac{\chi(x)^2}{\sin m\,\theta}\int_0^\infty dz\epsilon^{-(\chi_1(x)-\cos m\theta)z}\sin(z\sin m\theta).$$

The general term of the series included under the signs of definite integration is now of the form

$$P\psi(x)\epsilon^{\alpha\psi(x)}\epsilon^{-\frac{\beta}{\chi(x)}}\chi(x)^{\frac{r}{m}}a^x,$$

belonging to a class which I have considered in my former memoir.

Let us now consider the series

$$\sqrt[n]{\phi(0)}^{\sqrt[m]{\psi(0)}}+a.\sqrt[n]{\phi(1)}^{\sqrt[m]{\psi(1)}}+a^2.\sqrt[n]{\phi(2)}^{\sqrt[m]{\psi(2)}}+\ldots$$

$$+a^x.\sqrt[n]{\phi(x)}^{\sqrt[m]{\psi(x)}}+\&c.,$$

$\phi(x)$ and $\psi(x)$ being rational functions of (x).

$$\sqrt[n]{\phi(x)}^{\sqrt[m]{\psi(x)}}=\frac{1}{\Gamma\sqrt[m]{\psi(x)}}\int_0^1 u^{\frac{1}{\sqrt[n]{\phi(x)}}-1}\left(\log_\epsilon\frac{1}{u}\right)^{\sqrt[m]{\psi(x)}-1}du,$$

where $\psi(x)$ must be supposed less than unity, in order that the following transformation may hold:—

$$\frac{1}{\Gamma\sqrt[m]{\psi(x)}}=\frac{\sin\pi\sqrt[m]{\psi(x)}}{\pi}\Gamma(1-\sqrt[m]{\psi x})$$

$$\sin\pi\sqrt[m]{\psi(x)}=\frac{1}{2\pi}\int_0^\pi\frac{d\theta(1-\sqrt[m]{\psi(x)^2})d\theta(\sin\pi\epsilon^{i\theta}+\sin\pi\epsilon^{-i\theta})}{1-2\cos\theta\sqrt[m]{\psi(x)}+\sqrt[m]{\psi(x)^2}}$$

$$=\frac{1}{2\pi}\int_0^\pi\frac{d\theta(1-\sqrt[m]{\psi(x)^2})F(\psi x\,\epsilon^{i\theta})F(\psi(x),\epsilon^{-i\theta})(\sin\pi\epsilon^{i\theta}+\sin\pi\epsilon^{-i\theta})}{1-2\cos m\theta,\ \psi(x)+\psi(x)^2},$$

where

$$F(\psi(x),\epsilon^{i\theta})\ =\psi(x)^{\frac{m-1}{m}}+\epsilon^{i\theta}\psi(x)^{\frac{m-2}{m}}+\ldots.,$$

$$F(\psi(x),\epsilon^{-i\theta})=\psi(x)^{\frac{m-1}{m}}+\epsilon^{-i\theta}\psi(x)^{\frac{m-2}{m}}+\ldots.;$$

also

$$\Gamma(1-\sqrt[m]{\psi(x)}=\int_0^\infty\epsilon^{-v}v^{-\sqrt[m]{(\psi x)}}dv.$$

The remainder of the process will be evident from the two former examples.

V. "On a Theorem concerning Discriminants." By J. J. Sylvester, F.R.S. Received May 27, 1865.

Let $F(a,b,c,d)=a^2d^2+4a^3c+4d^3b-3a^2b^2-6\,abcd$, and let a, b, c, d be four quantities all greater than zero, which make this function vanish.

(1) The cubic equation in x, $F(a,x,c,d)$ will have two positive roots (b,b_1); so $F(a,b_1,x,d)$ will have two such roots (c,c_1), $F(a,x,c_1,d)$ two such (b_1,b_2), $F(a,b_2,x,d)$ two such (c_1,c_2), and so on *ad infinitum*; we may thus generate the infinite series $b_1\,c_1\,b_2\,c_2\ldots\ldots\ldots$

Similarly, beginning with the equation $F(a,b,x,d)$, and proceeding as above, we shall obtain a similar series, $c', b', c'', b''\ldots$; and combining the two together, and with the initial quantities b, c, we obtain a series proceeding to infinity in both directions ..... $b''\,c''\,b'\,c'\,b\,c\,b_1\,c_1\,b_2\,c_2\ldots\ldots$

(2) The four quantities

$$\frac{\delta F}{\delta a},\ \frac{\delta F}{\delta b},\ \frac{\delta F}{\delta c},\ \frac{\delta F}{\delta d},$$

where F represents $F(a,b,c,d)$, will present one or the other of the three following successions of sign,

$$\begin{array}{cccc} + & - & + & - \\ - & + & - & + \\ 0 & 0 & 0 & 0 \end{array}$$

(3) When the last is the case, *i.e.* when the differential derivatives all

vanish, the quantities b, c remain stationary in the above double infinite series; in the two other cases, the b quantities and c quantities *continually* increase in one direction and *continually* decrease in the other, the increase taking place in that direction in which we must read the successions of sign of the derivatives of F so as to begin with passing from plus to minus.

(4) To the increase of b and c there is no limit, but to the decrease of each there is a limit, viz. $a^{\frac{2}{3}}\ d^{\frac{1}{3}}$ and $a^{\frac{1}{3}}\ d^{\frac{2}{3}}$ are the limits towards which the b and the c terms respectively converge.

I conclude with remarking that the above theorem is only a particular illustration, and the most simple that can be given, of a very wide theory relating to discriminants of all orders which springs as an immediate consequence from the principles involved in the theory of variation of algebraical forms referred to in the note which I had recently the honour of laying before the Society.

VI. "Some Observations on Birds, chiefly in relation to their Temperature, with Supplementary Additions." By JOHN DAVY, M.D. F.R.S., &c. Received May 26, 1865.

(Abstract.)

This paper consists of four parts:

In part first the author gives the results of his observations on the temperature of the common fowl (as many as sixty-two), made at different seasons of the year, showing that the temperature of this bird ranges from 107° to 109° Fahr. *in recto*; that that of the male is a little higher than that of the female, and of both, higher in summer than in winter.

He states that he was induced to pay so much attention to the temperature of the common fowl, from Mr. Hunter having assigned it a temperature no higher than between 103° and 104°, a degree reached by some of the mammalia, and even exceeded.

The second part contains the results of the author's experiments on the air expired by a certain number of birds, and on the air contained in their air-receptacles and bones. They are introduced with some observations on the length of time birds are capable of retaining life under water, from which it appears that it differs greatly in different species, varying from ten minutes, as in the instance of the duck, to half a minute, as in the instance of the owl.

From the analysis of the air expired in the act of drowning, it would appear that there is a certain loss of carbonic acid, equivalent to the proportion of oxygen less than exists in the atmospheric air expired,—a loss, it is inferred, owing to absorption by the blood of the gas which has disappeared, as indeed is indicated by the darkness of colour of this fluid, and confirmed by the effects of exhaustion by the air-pump.

A deficiency, too, of carbonic acid was found in the air of the air-recep-

tacles and in the air contained in the bones, attributable to the same cause.

Of the third part, the subject is pulmonary and cutaneous aqueous exhalation. From the facts brought forward, and the experiments described, there appears to be proof afforded that birds perspire little and cool slowly, and consequently that their high temperature is partly owing to these two circumstances—the one (the latter) attributable to their clothing of feathers, the other (the former) to the little vascularity of their cutis.

The subject of the fourth part is the kidneys and their excretion. As these organs are proportionally large in birds, and as their excretion is very much less liquid than that of the mammalia, it is presumed that, from its carrying off less heat, it may be considered an element in the problem of the high temperature of birds, and that as the compound excreted is chiefly urate of ammonia, it may conduce to a less expenditure of oxygen than if urea were a constituent of their urine.

In conclusion the author suggests that the high temperature for which most birds are remarkable (not all, there being certain exceptions) may be due to a combination of circumstances, some positive, some negative,—the positive chiefly the conversion of oxygen into carbonic acid, the negative those conditions influential mainly by economizing the heat produced or checking its escape. Further, it is conjectured that there may be other ancillary conditions, such as a powerful heart ensuring a rapid circulation, the peculiar structure of the blood-corpuscles, and the little viscidity of the blood of birds.

Some remarks are added on the pneumatic system of birds, so distinctive of their class, with conjectures on its uses, these in part obscure and seeming to require further research for their elucidation.

The supplementary additions are given chiefly in a tabulated form, of which there are five:

The first contains a list of birds examined, altogether 64, of which number 22 were found to have air in some of their bones, and 42 to be without air in them.

In the second the weight of a certain number of species is given (39), and the weight of their feathers, and in most instances of their bones.

In the third the weight of the principal bones of a selected number of species (27) is stated.

In the fourth the composition of some of the bones, as determined by calcination.

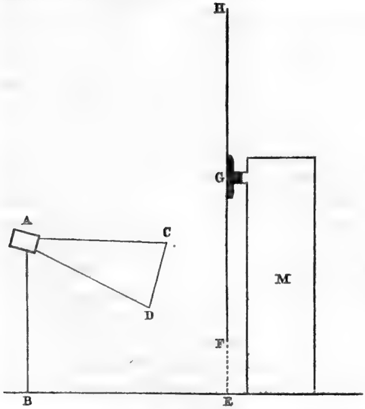
And in the fifth the weight of the principal organs of five species.

With the exception of the last, some brief remarks are appended to each Table.

VII. "On the Heating of a Disk by rapid Rotation *in vacuo*." By BALFOUR STEWART, M.A., F.R.S., and P. G. TAIT, M.A. Received June 1, 1865.

1. The authors were led, by certain views which they entertain regarding the loss of energy by a body, to make experiments in order to test these views, and about the end of 1863 they obtained results in air, which encouraged them to have constructed an apparatus wherewith to procure rotation *in vacuo*.

2. The apparatus for this purpose was devised and executed by Mr. Beckley, mechanician at the Kew Observatory, at which place the experiments about to be described were made. In this apparatus a slowly revolving shaft is carried up through a barometer tube, having at its top the receiver which it is wished to exhaust. When the exhaustion has taken place, it is evident that this shaft will revolve in mercury. In the receiver the shaft is connected with a train of toothed wheels, and ultimately causes a circular disk to revolve 125 times for each revolution of the shaft. The disks used have a diameter of 13 inches, and their plane is vertical. Two insulated wires, connected with a Thomson's reflecting galvanometer, are carried through two holes in the bed-plate of the receiver, and are then connected with a thermo-electric pile, having the usual reflecting cone attached to it. The outside of the pile, and of its attached cone, is wrapped round with wadding and cloth, so as to be entirely out of the reach of currents of air. The vacuum-gauge is on the siphon principle; it was constructed by P. Adie, and there is every reason to believe that it is perfectly deprived of air. The following figure will render the arrangements clear:



A B= 6 inches.
B E= 8 „
E G= 8¼ „
F H=13 „ (diam. of disk.)
A C= 5¼ „
C D= 2½ „
M. Multiplying gear.

and it is only necessary to add that the whole is covered over with an air-tight glass shade, 15 inches in diameter and 16 inches high.

3. The deflection of the galvanometer-needle, produced by heating the pile, is recorded by means of a small mirror attached to the needle, which, as it moves, causes the reflected image of a line of light to travel over a graduated scale. The galvanometer-needle is rendered very nearly astatic by means of an auxiliary magnet; and this arrangement can be made so sensitive that if the temperature of the disk, exposed to the cone as in the figure, were to rise 1° Fahr., this would be denoted by a change in the position of the line of light equal to fifty divisions of the scale.

4. In these experiments the disk was rotated rapidly for half a minute, and a heating effect was, in consequence of this rotation, recorded by the pile. The object of this paper is to investigate the origin of this heating effect.

5. In this investigation the authors prefer discussing the effect produced on a metallic disk. The metal aluminium was chosen, from its lightness, so as to diminish the weight upon the bearings as much as possible. The reason for preferring a metallic disk is that the heat produced in this case affects the whole substance of the disk, and can thus be approximately measured. The disk of this metal employed was $\frac{1}{20}$th of an inch thick and 13 inches in diameter; it weighed ten ounces, and in most of the experiments it was covered with a coating of lampblack, applied by means of negative photographic varnish. In some of the experiments a plate of rock-salt, tightly secured in a brass fitting, was screwed upon the mouth of the cone. When this was done, a small piece of anhydrous baryta was placed within the cone to keep the inner surface of the salt dry, and a dish containing strong sulphuric acid was likewise placed in the receiver. Indeed the latter was always used; so that in the results obtained the residual air may be considered as nearly dry, and the surface of the disk, as well as that of the rock-salt, when this was used, nearly free from moisture.

Furthermore, in order to obviate the objection that the electric currents which take place in a revolving metallic disk might alter the zero of the galvanometer, the position of the line of light was read before the motion began, and immediately after it ceased, the difference being taken to denote the heating effect produced by the rotation. The turning was made in this way:—As soon as full speed was obtained, which might be about 10 seconds after beginning the motion, a chronometer was noted, and the handle was turned at a uniform rate for 30 seconds, and thereafter stopped as soon as possible. The most convenient speed *in vacuo* was found to be 20 revolutions of the handle, or 2500 of the disk, in 30 seconds. It is believed that the heating effect recorded may be considered as due to about 40 seconds at full speed.

6. The *thermometric* value of the indication given by the galvanometer was found in this way:—The disk was removed from its attachment and laid upon a mercury-bath of known temperature. It was then attached to its spindle again, being in this position exposed to the pile, and having a temperature higher than that of the pile by a known amount. The deflec-

tion produced by this exposure being divided by the number of degrees by which the disk was hotter than the pile, we have at once the value in terms of the galvanometric scale of a heating of the disk equal to 1° Fahr.

7. The following sets of experiments were made with blackened aluminium disk and rock-salt in the cone.

No. of set.	No. of observations in each set.	Time at full speed.	No. of turns of handle at full speed.	Heat indication. ° Fahr.	Tension of air in inches.
I.	3	30	20	0·85	0·3
II.	4	30	20·5	0·87	0·3
III.	4	30	20	0·81	0·3
IV.	3	30	20	0·75	0·65

8. A piece of wood precisely similar to the rock-salt plate was next inserted into the fitting of the latter, and after rotation there was no indication whatever. Hence the above effect (art. 7) is due to radiant heat, and not to currents of heated air reaching the pile. Again retaining the rock-salt, the interior of the cone was covered by black paper, and the effect upon the pile was very much diminished: this also goes to prove that the effect (art. 7) is due to radiant heat; and it now remains to discover whether this radiant heat comes from the rock-salt or from heated air, or from the surface of the disk.

9. The following sets of experiments were made with blackened aluminium disk, but without rock-salt.

No. of set.	No. of observations in each set.	Time at full speed.	No. of turns of handle at full speed.	Heat indication.	Tension of air in inches.
V.	3	30	20	0°·92	0·37
VI.	3	30	20	0·93	0·60

And when a black paper cover was introduced into the cone, other things remaining as before, the indications of the galvanometer were greatly diminished. The effect produced without rock-salt is therefore also a radiant heat effect; and as the indications (in terms of temperature) are as large as when rock-salt was used, we may conclude that the effect of art. 7 was to no perceptible extent due to heating of the rock-salt, otherwise it would have been diminished when the plate of rock-salt was taken away. Besides, as rock-salt is a bad radiator and a good absorber of its own heat, the plate would have had to be heated perhaps as much as 15° or 20°, in order to furnish a radiation equal to 0°·8 from the disk. On both these accounts it is impossible to believe that the effect was due to heating of the rock-salt.

10. Nor is it probable that the heating effect is due to radiation from heated air, since in order that nearly dry air of such a tenuity might give such a radiation, it would require to be heated enormously. But another proof that the effect is not due to air is afforded by removing the black from the aluminium disk and leaving it a rough metallic surface, when

the indication afforded by the galvanometer is reduced to about one-fourth of the amount with the blackened disk.

11. It only remains that the heating effect proceeds from the disk, and since the heat-indication afforded by the galvanometer-needle remains nearly constant for some time, this effect must be due to the heating of the whole substance of the disk.

12. Presuming, therefore, that the entire substance of the disk is heated, the next point is to ascertain the cause of this heating effect.

Now, in the first place, it cannot be due to conduction of heat from the bearings, for in some of the experiments the disk was insulated from its bearings by means of a plate of ebonite, and the result was the same.

Again, it is not due to revolution under the earth's magnetic force, for Professor Maxwell has kindly calculated the effect due to this cause under the conditions of the experiment, and he finds it infinitesimally small. Nor is the effect due to the condensation of vapour of water upon the surface of the disk. In some of the experiments, when the vacuum was newly made, there appeared to be a strictly temporary effect, due probably to moisture, which *increased* the range of the needle, but only during the time when the motion was taking place, for it very soon assumed its permanent position. In other experiments, when the air was very dry, there appeared to be a temporary cold effect of a similar description; but in all cases when the vacuum was kept long enough for the sulphuric acid to act, the only effect was a permanent one in the direction of heat, and this is that which has been described in these experiments. This permanent heating effect cannot, therefore, be due to the condensation of aqueous vapour, and indeed it is impossible to suppose that in the presence of sulphuric acid so much vapour should remain suspended in air of so low a tension as to produce a permanent effect so very considerable by its deposition.

13. In this endeavour to account for the heating effect observed, it would appear that we are reduced to choose between one of two causes, or to a mixture of the two.

(1) It may be due to the air which cannot be entirely got rid of.

(2) It is possible that visible motion becomes dissipated by an ethereal medium in the same manner, and possibly to nearly the same extent, as molecular motion, or that motion which constitutes heat.

(3) Or the effect may be due partly to air and partly to ether.

14. Now, if it be an air effect, it is not one which depends upon the mass of air. For (art. 7) the effect for a vacuum of 0·3 in. is as large as for one of 0·65 in.; and also (art. 9) the effect for a vacuum of 0·37 in. is as large as for one of 0·60 in.; and further, in some approximate experiments, the effect produced upon a wooden disk, in a vacuum of 4·0 in. and 2·0 in., was found to be the same as in one of 0·5 in., or very nearly so. It may therefore be presumed that only a very inconsiderable portion of the effect observed depends upon the mass of air left behind. It would, however, appear, from the views of Professor Maxwell and Mr. Graham, that there is

another effect of air, namely, fluid friction, the coefficient for which they believe to be independent of the tension; and as far, therefore, as this effect is concerned, little is gained by diminishing the amount of the residual air. It would appear, however, that the fluid friction of hydrogen is much less than that of atmospheric air; so that, were the heating effect due to fluid friction, it ought to be less in a hydrogen vacuum. An experiment was made with this purpose; and, other circumstances being precisely similar, it was found that in a hydrogen vacuum the heating effect due to rotation was 22·5, while in an air vacuum it was 23·5. These numbers may probably be considered as sensibly the same, and this experiment would therefore appear to denote that the effect is not due to fluid friction.

15. The authors, in submitting these remarks to the Royal Society, do not suppose that their experiments have yet conclusively decided the origin of this heating effect, but they hope by this means to elicit the opinions of those interested in the subject, which may serve to direct their future research.

VIII. "On the Fossil Mammals of Australia.—Part II. Description of an almost entire Skull of *Thylacoleo carnifex*, Ow." By Professor Owen, F.R.S., &c.

(Abstract.)

In this Part the author gives additional cranial and dental characters of the extinct marsupial carnivore, *Thylacoleo,* deduced from examination of better-preserved fossils, obtained from freshwater deposits in Darling Downs, Queensland, Australia.

The forepart of the skull, wanting in the first-described specimen from similar deposits in the province of Victoria, is preserved in the present specimen, showing the premaxillary bones, which are relatively larger than in placental felines. Each bone has three teeth, of which the foremost is developed into a tusk, the second and third being very small. There is no canine, or no tooth developed as a laniary in the maxillary bone. In the short extent of the alveolar border of this bone between the great carnassial molar and the maxillo-premaxillary suture, there are two approximate small round sockets, which lodged either one double-rooted tooth or two small single-rooted teeth. But dental development has mainly expended itself upon the perfection of a pair of laniary incisor tusks, in both upper and lower jaws, for piercing, tearing, and holding, and a pair of carnassials in both jaws for flesh-cutting. These, in the present specimen, closely agreed with those described in the former one, but were more worn: they are the largest examples of these peculiarly modified shear-blade teeth in the mammalian class. Although the tusks are incisors—not, as in placental carnivora, canines—they possess, through the singular shortness of the facial part of the skull in *Thylacoleo,* the same mechanical advantage, in their proximity to the biting-power of the enormously deve-

loped temporal muscles, as in *Felis*. In the lower jaw there is, anterior to the carnassial, either a socket for a small double-rooted premolar, or two approximate sockets for as many single-rooted ones; and, as in the upper jaw, these cavities do not range in the same longitudinal line with the carnassial, but extend obliquely inward and forward, from the inner side of its fore part. There is no other alveolus in the lower jaw between the premolar one and that of the large lower tusk. The small 'tubercular' molar on the inner side of the hind end of the upper carnassial, and the two 'tuberculars' behind the lower carnassial, are indicated by their sockets in the present specimen. The author sums up, from acquired data, the dental formula of *Thylacoleo* as follows:—Incisors $\frac{3-3}{1-1}$, Canines $\frac{?}{?}$, Premolars $\frac{1-1}{1-1}$ or $\frac{2-2}{2-2}$, Carnassials $\frac{1-1}{1-1}$, Tuberculars $\frac{1-1}{2-2}$. Of the incisors, the foremost above are long and large tusks, like the pair below: of the other teeth, the carnassials, of unusually large size, are functioned as flesh-cutters, and the small tuberculars would serve for pounding gristle or tendon, as in *Felis*: the premolars indicated by sockets, and the small upper incisors, represent a remnant of the dental family type under its extreme adaptive modifications in *Thylacoleo*.

In the rest of the skull of the subject of the present Part, many particulars are yielded in addition to those deduced from the fragmentary fossils which indicate the genus. They confirm the deductions of the marsupial nature of the large extinct Australian carnivore; determine the alternative expressed in the author's first communication as to the homologies of the inferior tusks, and show that the genus *Thylacoleo* ranges, not with the series now including *Didelphys*, *Dasyurus*, and *Thylacynus*, but with the Diprotodont group, more eminently characteristic of the Australian continent, and which is at present represented by, or reduced to, the genera *Phascolarctos*, *Phalangista* with its subgenera, *Macropus* with its subgenera, and *Phascolomys*. The carnassial of *Thylacoleo*, in its large proportional size, absence of the tubercular part, and indications of subvertical groovings of the enamel, most closely resembles that tooth of the more ancient marsupial carnivore *Plagiaulax*, and is associated, in the lower jaw, as in that genus, with two small posterior tuberculars, one or two small premolars, and one large incisive tusk, similarly directed obliquely upward and forward. Few facts in mammalian palæontology are more interesting and suggestive than the occurrence in our hemisphere, during secondary geological periods, of Marsupial forms, which find their nearest representatives in existing or tertiary extinct *Marsupialia* of the continent of our Antipodes.

The present Part of the author's series of Papers on Extinct Australian Mammals is illustrated with drawings of the entire skull of the *Thylacoleo carnifex*.

IX. "On the Normal Circulation and Weight of the Atmosphere in the North and South Atlantic Oceans, so far as it can be proved by a steady Meteorological Registration during five Voyages to India." By Captain HENRY TOYNBEE. Communicated by Major-General SABINE, President. Received June 8, 1865.

Having lately made five voyages to India, leaving England on the 1st of July and returning early in April, I have observed the recurrence of certain facts relating to the weight and circulation of the air in the same part of the world at the same seasons of the year, from personal registration of the barometer, wet- and dry-bulb thermometers, direction and force of the wind, &c., five times daily.

These five voyages have carried us through the Atlantic Oceans from 50° N. to 40° S. lat. in the months of July and August; again returning home, we have passed from 34° S. lat. to 50° N. lat. during the months of February and March each year.

The accompanying diagrams * show the height of the barometer at noon in each degree of latitude; and as we were not in each degree exactly at noon, interpolation has been used: for instance, if in 24° N. lat. at noon the barometer were 30·12 inches, and in 22° N. lat. it were 30·10 inches, it has been called 30·11 inches in 23° N. lat. This plan is not to be trusted, however, on the polar side of the trade-winds, where the barometer is constantly undergoing change, depending upon a series of independent gales, of which something will be said by and by; but it seems to give very correct results in and between the trade-winds, where the height of the barometer had long been noticed to depend chiefly upon the latitude and season of the year

We will first allude to the five diagrams representing the outward passage, the two upper ones lying to the west, or outside the Cape Verde Islands, the three lower ones to the eastward, or between them and Africa.

The first facts they prove are, that the barometer at this season ranges lower to the eastward than to the westward of the Cape Verdes, the N.E. trades extending further south to the westward, and the S.W. monsoon (which at this season blows between the trades) sets in further north, and blows stronger the nearer to Africa.

We are told that the Great Sahara Desert being heated by the sun of the northern summer causes an upward current of air, which draws in the air from the sea to restore equilibrium, just as the heated lands in India during the same season cause a S.W. monsoon in the Bay of Bengal, where a N.E. trade would otherwise prevail. Our barometer diagrams show this by being lower near Africa, and gradually rising as the distance from the demand is increased. Again, in about 13° N. lat., where the S.W. monsoon commences, it is always much more from the west than it is further southward, where the wind draws to the south, and very generally turns

* The Barometric Curves and Track-Chart are preserved for reference in the Archives.

into the S.E. trades without any intervening calm. In fact this heated part of Africa seems at this season to have the power of bringing the N.E. trades to an end in about 17° N. lat. between the islands and the main, instead of 13° N. lat. outside, and of causing an indraft from the westward; it also gradually turns the S.E. trade which blows near Africa into a S.W. wind, which we may suppose finds its way into the upper stratum of air over this heated land. Part of this S.W. monsoon seems to be formed of the damp cloudy air which exists in the doldrums, whilst the rest is evidently formed of clearer air—another evidence that it is part of the S.E. trades.

It would be interesting to treat in a similar way a few logs of American ships leaving in July, and supplied with standard instruments, since they might show how far to the westward the barometer continues to range higher, and we have Maury's works to prove that the N.E. trades do extend nearer to the equator in more western longitudes.

Having thus considered the curves of the outward passage from England with respect to their difference when further east or further west, we will view them in a north and south direction. It will be noticed that the lowest barometer occurs in the belt of doldrums, between the trades; and by comparing the outward with the homeward route it will be seen, as is well known, that this belt is further north in July than in March. It is interesting, however, to see that the lowest barometer travels north or south with the belt of doldrums, showing that its cause must be sought for there, and not in centrifugal force, which might be supposed to fix it at the equator.

The sailor is naturally led to ask how it is that the barometer is lower here, a zone towards which two trade-winds are pouring in an immense body of air along the earth's surface, and in nearly opposite directions; for near the equator the trades draw more north and south. We suppose there can be but one answer, viz. that here the air rises, and forms those two upper currents which rush towards the poles, above and counter to the trade-winds. Maury tells us that the so-called African dust is really South American, and that much more rain falls in the northern than in the southern hemisphere, from which he argues that the air which formed the S.E. trades, having traversed more sea and picked up more moisture, rises in these doldrums, and travels to the N.E. above the N.E. trades; and *vice versâ*, that the N.E. trades travel to the S.E. above the S.E. trades. He does not say how they pass each other, neither can we, but we have strong evidence of a current of air travelling above and in opposite direction to the trade-winds, because we generally see the high clouds travelling in that direction. We have, however, as it were, even seen the air ascending; for on the 15th of March, 1865, in 4° 18′ N. lat. and 20° 33′ W. long., when we had light fleecy clouds passing over us from the N.E., and we lay becalmed and roasting, longing for the trades, my chief officer came and reported to me with a hopeful countenance that he had seen these light

fleecy clouds travelling from the N.E. None but those who have experienced these calms can imagine how anxiously wind is looked for: to the N.E. of us there was an arch of clouds in the sky extending from the S.E. to the N.W. points of the horizon, with a calm and low barometer on the south side of it where we were, and (as we found afterwards) the N.E. trade and a higher barometer on its north side; therefore the arch of clouds was probably formed by the condensation of moisture as the air rose, while we lay becalmed at the foot of the inclined plane of still air, up which the N.E. trade was just commencing its ascent.

Travelling south across the equator, it will be noticed how uniformly the barometer rises until we arrive at the southern limit of the S.E. trades; but on referring to the homeward curves in February, it will be noticed how much lower the barometer ranged then than in August. The homeward route through the Atlantic differing much from the outward, does this difference of barometer arise from difference of seasons or difference of longitude? By comparing the routes near the equator, where they come very close to each other, and where the difference of height in the quicksilver is as great as in any other part, I am led to think that it depends upon the different seasons.

Whilst speaking of the homeward route, it is interesting to remark how on leaving the Cape of Good Hope we invariably had a valley, as it were, in the atmosphere, which quickly rose as we sailed to the N.W., even though we may have started in a south-easter, which is the high-barometer wind in these latitudes. I had noticed that after rounding Cape Agulhas with a south-easter and high barometer, the column fell suddenly after rounding the Cape of Good Hope, though the S.E. wind continued; and I suppose that the fall is caused by the air's ascending as it comes in contact with the high land: the curves seem to support this opinion.

It will be noticed that at both seasons of the year there is a heaping-up of the air at the polar end of each trade, in the place where Maury tells us that two upper currents come to the surface of the earth; the one we have already alluded to, which comes from the equator towards the pole, moving above the trades, clouds proving its existence; the other, Maury tells us, rises at the pole, and travels as an upper current, above the strong westerly winds which prevail in high latitudes, towards the equator; it can hardly be expected to have many clouds, he says, as its moisture must have been condensed by cold before rising at the pole, so that it becomes cold dry air.

We may ask what evidence the sailor can give for this theory as deduced from observation.

First, then, from these heaps of air he finds two surface winds blowing in opposite directions: the one moving towards the equator is cool, dry, and heavy, the other moving towards the pole is warm, damp, and light. He may well say, If two surface winds blow in opposite directions from this heap of air, there must be air brought to it by an upper current or

currents to keep up the heaping; but he may naturally ask, how do I know that an upper current comes from the pole? First, because the prevailing surface winds in high latitudes blow towards the pole, which air must return; and secondly, because the trade-winds are composed of cool dry air, which could not have come from the equator: here there is pretty good evidence that two upper currents come to the surface of the earth in these zones where the air is heaped up, and again, that in dipping to the surface by some unknown means they cross each other, as Maury conjectured.

Perhaps a few words may be desirable as to the manner in which the westerly winds which blow in high latitudes appear to draw the air from the heaps above mentioned. Here we will refer to our experience in 40° S. lat., where the normal circulation of the air is less interfered with by the land.

This parallel of latitude is subject to a series of gales which commence at N. and end at N.W. or W. As the north wind sets in, the barometer falls, the air becomes warm, damp, and cloudy; the wind gradually draws round to the N.W., after a time rain accompanies the wind, the barometer continues to fall, often fast, until in a heavy shower of rain the wind shifts to the west, when the barometer immediately rises, generally followed by a strong breeze from the westward, which decreases as the quicksilver rises, very often settling down into a calm. After a few hours the north wind sets in again, with a falling barometer, and a repetition of the whole series takes place.

One is naturally led to ask why the trade-wind draws air from this heap in a regular continuous stream, when these gales are fitful. May it not be because in the direction in which the trade moves the meridians diverge and give plenty of room for the flow, whereas the westerly winds have converging meridians which seem to check the progress of the air. These fitful gales have always led me to think that the air was checked in its course. If further south, say in 50° S. lat., the wind continues steady from the west (as Maury leads us to suppose is the case), then this zone of 40° seems to act as a reservoir for the westerly winds, being constantly refilled and steadily drawn off, only the stream into the reservoir is freer than that which runs out.

Now if we consider that these gales are composed of the warm damp air which come to this heap from the equator above the S.E. trades, descending to the surface of the earth and travelling towards the pole, their westing is accounted for by the change in the diameter of the circular route which the air has to describe in accompanying the earth in its revolution. These gales changing from N. to N.W. and W. have been treated as the N.E. quarters of southern hemisphere cyclones; and we read in the 'Nautical Magazine' of a ship's having hove to to allow one of them to pass; but if, as we suppose, they form part of the normal circulation of the air, it seems useless to heave to to avoid them. The source of these gales

being to the north of them is a sufficient reason why the wind does not change to south of west. The polar-wind gales which are experienced in these high latitudes, seem to derive their air from that upper current returning from the pole, part of which sometimes makes its downward way to the surface in high latitudes, especially in spring.

The gales of the southern hemisphere, just remarked upon, have their exact counterpart in the high latitudes of the northern hemisphere, though I have not noticed them to be so constant, perhaps on account of there being much more land in the northern hemisphere. Still all seamen know how, after getting north of the N.E. trades, we look for the wind to come from S., S.W., and W., with warm air and rain.

These curves, and the arguments deduced from them, seem to favour Maury's theory of the circulation of the air; where he supposes two rising currents we have a low barometer, and where he supposes two descending currents we find a high barometer; but they are also suggestive, and a series made with standard instruments for each month in the year might lead to most useful discoveries as to the normal circulation, and its disturbance by the effect of land. How strikingly these curves prove the uniform state of the atmosphere in those parts of the Atlantic between the trades, at the same seasons of the year! especially in contrast with their sudden distortions on the polar side of the trades, where their irregularities resemble the waves of the sea in the same latitudes, which may in fact be called the resultants of these distortions. Similar curves, outward and homeward, deduced from the same logs, between the latitudes of 40° South to 20° North, in the Indian Ocean and Bay of Bengal, would, I think, give interesting results, and I hope some day to work at them.

A track-chart accompanies these remarks, showing the routes inside and outside the Cape Verde Islands, together with a homeward-bound route, thus showing the longitude in which each degree of latitude has been crossed.

X. "On the Sextactic Points of a Plane Curve." By William Spottiswoode, M.A., F.R.S., &c. Received June 15, 1865.

(Abstract.)

The beautiful result given by Professor Cayley in the Proceedings of the Royal Society (vol. xiii. p. 553), and deduced, as I understand, by the methods of his memoir "On the Conic of Five-pointic Contact" (Philosophical Transactions, vol. cxlix. p. 371), led me to inquire how far the formulæ of my own memoir "On the Contact of Plane Curves" (Philosophical Transactions, vol. clii. p. 41) were applicable to the solution of the present problem.

The formulæ in question are as follows: if $U=0$ be the equation of the curve, H its Hessian, and $V=(a, b, c, f, g, h \between x, y, z)^2=0$ that of the conic of five-pointic contact; and if, moreover, α, β, γ being arbitrary constants,

$$\delta = ax + \beta y + \gamma z$$

$$\Box = (v\gamma - w\beta)\partial_x + (wa - u\gamma)\partial_y + (u\beta - va)\partial_z,$$

then, writing as usual

$$\partial_x U = u,\ \partial_y U = v,\ \partial_z U = w,\ \partial_x H = p,\ \partial_y H = q,\ \partial_z H = r,$$

$$\partial^2 U = u_1, \ldots \partial_y \partial_z U = u', \ldots$$

$$A = v_1 w_1 - u'^2, \ldots F = v' w' - u_1 u', \ldots$$

the values of the ratios $a : b : c : f : g : h$ are determined by the equations

$$V = 0,\ \Box V = 0, \ldots \Box^4 V = 0.$$

Now, if at the point in question the curvature of U be such that a sixth consecutive point lies on the conic V, the point is called a sextactic point; and the condition for this will be, in terms of the above formulæ, $\Box^5 V = 0$. From the six equations $V = 0$, $\Box V = 0, \ldots \Box^5 V = 0$, the quantities $a, b, \ldots h$ can be linearly eliminated, and the result will be an equation which, when combined with $U = 0$, will determine the ratios of $x : y : z$, the coordinates of the sextactic points of U. But the equation so derived contains (beside other extraneous factors) the indeterminate quantities a, β, γ, to the degree 15, which remain to be eliminated. Instead, however, of proceeding as above, I eliminate a, β, γ beforehand, in such a way that $V = 0$, $\Box V = 0$, $\Box^2 V = 0$ take the form

$$\frac{\partial_x V}{u} = \frac{\partial_y V}{v} = \frac{\partial_z V}{w} = \frac{\Delta V}{\varpi H};$$

and more generally if $W = 0$, representing any one of the series $V = 0$, $\Box V = 0, \ldots$ from which a, β, γ have been already eliminated, the equations $W = 0$, $\Box W = 0$, $\Box^2 W = 0$ are replaced by

$$\frac{\partial_x W}{u} = \frac{\partial_y W}{v} = \frac{\partial_z W}{w} = \frac{\Delta W}{\varpi H},$$

where H is the Hessian of U, ϖ a numerical factor, and

$$\Delta = (A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2.$$

Proceeding in this way, I obtain a result free from a, β, γ in the three forms,

$$\begin{vmatrix} \partial_x(uX - xP) & \partial_x(uY - yP) & \partial_x(uZ - zP)u \\ \partial_y(uX - xP) & \partial_y(uY - yP) & \partial_y(uZ - zP)v \\ \partial_z(uX - xP) & \partial_z(uY - yP) & \partial_z(uZ - zP)w \\ \Delta(uX - xP) & \Delta(uY - yP) & \Delta(uZ - zP)\varpi_2 H \end{vmatrix} = 0,$$

$$\begin{vmatrix} \partial_x(vX - xQ) & \partial_x(vY - yQ) & \partial_x(vZ - zQ) & u \\ \vdots & \vdots & \vdots & \vdots \end{vmatrix} = 0,$$

$$\begin{vmatrix} \partial_x(wX - xR) & \partial_x(wY - yR) & \partial_x(wZ - zR) & u \\ \vdots & \vdots & \vdots & \vdots \end{vmatrix} = 0,$$

where

$$X = vr - wq,\ Y = wp - ur,\ Z = uq - vp,$$
$$P = u_1\,X + w'\,Y + v'Z,$$
$$Q = w'\,X + v_1 Y + u'Z$$
$$R = v'\,X + u'\,Y + w_1\,Z,$$

and ϖ_2 is a numerical factor.

Each of these equations is of the degree $18u - 36$ in the variables; but it is shown in the paper that they are all divisible by H, and that they further differ only in respect of the several factors u^3, v^3, w^3. Dividing these out, the degree of the result is reduced to

$$(18n - 36) - 3\,(n - 2) - 3\,(n - 1) = 12n - 27,$$

as it should be. I have not thought it necessary to reduce the expressions completely, as the form of the result given by Professor Cayley leaves nothing to be desired, and the point specially considered here is the degree of the equation. At the same time, the reductions necessarily effected in the course of the proof of the extraneous factors are sufficient to indicate that the formulæ of the present memoir would lead to an equation of the same form as that given by Professor Cayley.

XI. "Products of the Destructive Distillation of the Sulphobenzolates. No. I." By John Stenhouse, LL.D., F.R.S., &c. Received June 14, 1865.

Preparation of Sulphobenzolic Acid. Purification of the Benzol.

As most specimens of benzol met with in commerce, even when rectified, contain impurities besides toluol and the other homologues of benzol, I have generally found it necessary to submit it to purification before using it for the preparation of sulphobenzolic acid. The commercial article boiling between 80° and 90° C., was mixed with about one-twentieth of its bulk of concentrated sulphuric acid, and digested for eight or ten hours in a flask furnished with a long condensing-tube. By this means a considerable amount of the impurities contained in the crude benzol were converted by the acid into a black gelatinous mass similar in appearance to that obtained in the preparation of olefiant gas, a large quantity of sulphurous acid gas was given off, and the impure benzol acquired a reddish-brown or dark purple colour. It was separated from the black mass, washed with a small quantity of water, and again heated once or twice with concentrated acid, but for a shorter time than at first, until fresh acid when heated with it ceased to become dark-coloured. In this operation the benzol loses from 10 to 20 per cent., according to the amount of impurity present, and small quantities of sulphobenzolic acid are produced.

Conversion of Benzol into crude Sulphobenzolic Acid.

This acid may be prepared by the process given by Mitscherlich, which consists in adding benzol to fuming oil of vitriol contained in a flask, as long as it dissolves, with agitation and frequent cooling. Notwithstanding

Mitscherlich's statement* that he has "succeeded as little as Faraday in combining benzol with ordinary strong sulphuric acid," I have found it advisable, when large quantities of sulphobenzolic acid are required, to treat the purified benzol with ordinary commercial acid.

Concentrated sulphuric acid and purified benzol, in the proportion of about four measures of the former to five of the latter, were placed together in a flask furnished with a long condensing-tube, and heated on a sand-bath for eight or ten hours. The flask in which the digestion is performed should be very large in proportion to the quantity of benzol employed, so that an extensive surface is exposed to the action of the acid †.

Sulphobenzolates.

The crude sulphobenzolic acid obtained by either of the above methods was separated from the uncombined benzol, and a quantity of water, about twenty times the bulk of the sulphuric acid originally employed, was added to it. This solution, which has a small quantity of sulphobenzene, $C_{12} H_{10} SO_2$, suspended in it, was heated to the boiling-point, neutralized with chalk, diluted with ten parts more water, and after being boiled for a few minutes, was filtered from the sulphate of calcium. The clear and slightly coloured filtrate is a solution of *sulphobenzolate of calcium*, $Ca C_6 H_5 SO_3$, from which the salt may be obtained by sufficient concentration. *Sulphobenzolate of barium* may likewise be prepared in a similar manner to the calcium salt, substituting carbonate of barium for chalk. The *sulphobenzolates of the alkaline metals* are readily obtained by precipitating the solution of the calcium salt by the carbonate of the desired metal, and evaporating the solution. By this process purified benzol yielded nearly twice its weight of sulphobenzolate of sodium. The *sulphobenzolates of copper, zinc,* &c., are best prepared by precipitating the solution of the barium compound by solutions of their sulphate. The copper salt is usually described in handbooks, on Mitscherlich's authority ‡, as forming fine large crystals. I have only been able to obtain it, whether from water or spirit, in very small crystals, which are exceedingly soluble.

Decomposition of Sulphobenzolate of Sodium.

The sodium-salt, after being reduced to powder and thoroughly dried, was introduced into a copper flask furnished with a bent tube, and submitted to destructive distillation, when an oily body covered with a layer of water condensed in the receiver, and a considerable quantity of carbonic and some sulphurous acid gas were evolved, carbonaceous matter and carbonate of sodium remaining in the retort. In order that the operation may proceed rapidly, and in the most advantageous manner, the quantity of sub-

* Pogg. xxxi. p. 284.

† A similar process has been employed by Gerhardt and Chancel in the preparation of sulphite of chlorobenzene, Compt. Rend. vol. xxxv. p. 690.

‡ Gmelin's Handbook, vol. xi. p. 156; Gerhardt, vol. iii. p. 72.

stance introduced into the retort should not exceed 25 to 30 grammes at each distillation.

Florence flasks can be used; but, owing to their bad conducting-power and the high boiling-point of the oil, the result is not so favourable. When the distillation is properly conducted in a copper retort, the dried sodium-salt yields from one-fourth to one-fifth of its weight of crude oil.

The crude oil was separated from the supernatant layer of water, and distilled in a retort furnished with a thermometer. It began to boil at 80° C., and then rose slowly to 110° C., the distillate between these temperatures consisting of a small quantity of water and benzol. When the water had all passed over, the boiling-point rose very rapidly to 290° C., at which temperature the greater portion of the liquid distilled over, leaving a black tarry residue in the retort. This black residue, when more strongly heated, gave a further quantity of an oily body, which, when rectified, first yielded the substance boiling at about 290° C., and above 300° C. a liquid which on standing some weeks deposited a small quantity of crystals. The quantity boiling between 290° and 300° C. was about two-thirds the weight of the crude oil.

The rectified oil between 290° and 300° C. was again distilled, when nearly the whole of it came over at 292°·5 C., the boiling-point being remarkably constant. After another rectification in a current of hydrogen, it was subjected to analysis—the carbon and hydrogen being determined by combustion with oxide of copper and a current of oxygen, and the sulphur by ignition with carbonate of sodium and oxide of mercury.

I. ·603 grm. oil gave 1·708 grm. CO_2 and ·292 grm. H_2O.
II. ·595 grm. oil gave 1·679 grm. CO_2 and ·288 grm. H_2O.
III. ·237 grm. oil gave ·302 grm. sulphate of barium.
IV. ·275 grm. oil gave ·354 grm. sulphate of barium.

	Theory.	I.	II.	III.	IV.	Mean.
C_{12}=144	77·41	77·25	76·98	..	..	77·12
H_{10}= 10	5·38	5·38	5·38	..	..	5·38
S = 32	17·20	..	..	17·48	17·51	17·49
186	99·99					

The formula $C_{12}H_{10}S$, deducible from these analyses, is that of sulphide of phenyl, or a body isomeric with it. When pure it is nearly colourless, having only a very faint yellow tinge, and an aromatic but slightly alliaceous odour. It has a high refractive power, sp. gr. 1·119, and boils at 292°·5. It is insoluble in water, very soluble in hot spirit, from which it partially separates on cooling, and is miscible in all proportions with ether, bisulphide of carbon, and benzol. Its alcoholic solution, when mixed with bichloride of platinum, gives a slight flocculent precipitate, which on standing resolves itself into a reddish-coloured oil. Nitrate of silver and chloride of mercury give no precipitate.

When the oil was treated with sulphuric acid, it dissolved in small quan-

tity, forming a red solution, which, on the application of a gentle heat, changed to a fine purple colour; this disappeared on raising the temperature, the whole of the oil dissolved, and a solution was obtained of a faint greenish-black tinge. When this solution was largely diluted with water it became nearly colourless, and on neutralization with chalk yielded (besides the sulphate) an organic calcium-salt, very soluble in water. The solution of the oil in sulphuric acid, when very strongly heated, blackened and gave off sulphurous acid gas. Solutions of the alkalies, whether aqueous or alcoholic, appear to have no action on the oil; but when heated with solid potash, it was decomposed with the production of compounds I am at present investigating.

Action of oxidizing Agents on Sulphide of Phenyl.—Sulphobenzolene. —When the oil $C_{12} H_{10} S$ was brought into contact with strong nitric acid, a very violent action ensued, accompanied by the copious evolution of nitrous fumes. The mixture was then boiled for an hour or two with occasional addition of fresh nitric acid, and the solution thus obtained poured into a large quantity of water, when a crystalline mass of a pale yellow colour was precipitated. This, when perfectly dried, was reduced to powder and washed with ether to remove a small quantity of adhering oil, and the partially purified product was recrystallized once or twice from benzol, and then several times from spirit, collecting apart the first portions which separate. By this means a substance in beautiful oblique prismatic crystals was obtained in a state of perfect purity, whilst in the mother-liquors there remained a large quantity of the same body mixed with a second substance crystallizing in long needles, which, however, formed but an inconsiderable portion of the whole.

Although this is the method by which I first prepared the above described substance crystallizing in oblique prisms, I have since employed a process which yields it with greater facility and in a much purer state. Ten parts by weight of water, five of concentrated sulphuric acid, and two of sulphide of phenyl, were placed in a flask furnished with a long condensing-tube, and to the mixture, kept boiling, three parts of acid chromate of potassium were added in small portions at a time. The digestion continued for twenty or thirty minutes, and the mixture was then allowed to cool. The green liquid was poured off from the cake of crystals, which, after boiling with water to free it from sulphuric acid, was dried. The nearly pure substance was then crystallized, once from benzol and once from alcohol, when it formed brilliant crystals which were perfectly pure. A trace of the second substance previously mentioned, crystallizing in long needles, was found in the benzol mother-liquors. By this last process, which is greatly preferable to the nitric acid one, the rectified oil yielded its own weight of crystals.

I. ·338 grm. crystals gave ·820 grm. CO_2 and ·144 grm. H_2O.
II. ·346 grm. crystals gave ·834 grm. CO_2 and ·144 grm. H_2O.

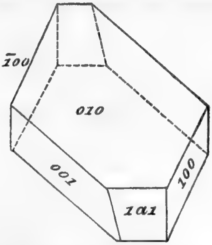
III. ·349 grm. crystals gave ·844 grm. CO_2 and ·156 grm. H_2O.
IV. ·241 grm. crystals gave ·259 grm. sulphate of barium.
V. ·362 grm. crystals gave ·385 grm. sulphate of barium.
VI. ·270 grm. crystals gave ·293 grm. sulphate of barium.

	Theory.	I.	II.	III.	IV.	V.	VI.
$C_{12}=144$	66·06	66·18	65·75	65·97	..	..	..
$H_{10}=10$	4·59	4·73	4·62	4·96	..	..	..
$S = 32$	14·68	..	..	..	14·74	14·59	14·89
$O_2 = 32$	14·68	..	..	..	..	..	..
218	100·01						

This substance was analyzed in the same manner as the sulphide of phenyl,—Nos. III., IV., V. being prepared by the nitric acid process, and Nos. I., II., VI. by oxidation with acid chromate of potassium and sulphuric acid.

The analysis of this substance leads to the formula $C_{12}H_{10}SO_2$, which is the same as that of the sulphobenzene of Mitscherlich. It differs greatly from that body, however, both in its chemical and physical properties. I shall therefore provisionally call it sulphobenzolene.

It forms oblique prisms, which are often of large size when prepared by crystallization out of benzol, in which it is rather soluble. The crystals obtained by both the processes above described were kindly measured for me by my friend Charles Brooke, Esq., F.R.S., who states they "are of the same form, the measurements corresponding exactly. They belong to the oblique prismatic system. The measurements must be considered as only approximative, the surfaces of the crystals being imperfect." No planes have been observed to determine the symbol *a* of the plane 1*a*1.



010 on 001 or 100......		94° 30′
010	$\bar{1}00$......	85 30
010	1*a*1......	108 20
001	$\bar{1}00$......	110 20

It is very soluble in hot spirit, from which it separates on cooling in a manner closely resembling the crystallization of chlorate of potassium. It is also soluble in ether, bisulphide of carbon, and slightly in boiling water, crystallizing out completely on cooling. It melts at 126° C., and distils at a much higher temperature.

Sulphobenzolene dissolves readily in concentrated sulphuric acid, and is not decomposed even when the solution is heated to the boiling-point.

Water precipitates the substance unchanged. Aqueous solutions of the alkalies appear to have no action on the crystals; but when they are heated with solid potash a powerful reaction takes place, with the production of new compounds.

When sulphobenzolene is digested for some time with a mixture of concentrated nitric and sulphuric acids, it dissolves, and red fumes are evolved. If a large quantity of water be now added to the mixture, a copious precipitate is obtained, difficultly soluble in hot alcohol, from which it crystallizes in minute needles.

Decomposition of Sulphobenzolate of Calcium.

When this salt was distilled in the manner described for the sodium compound, it underwent a similar decomposition—water and oil collecting in the receiver, and carbonic and sulphurous acids being evolved. In this instance, however, a very high temperature was required, and the quantity of oil obtained was much smaller than from the sodium-salt, being only about one-sixteenth the weight of the dry sulphobenzolate of calcium employed. The crude oil, when rectified, commenced to boil at 80° C., between which and 110° C. small quantities of water and benzol came over. The boiling-point then rose rapidly to 280° C., between which temperature and 300° C. about one-fifth of the original quantity of oil came over. Above 300° C. the distillate obtained became almost solid on cooling, consisting apparently of the same crystalline body of which a small quantity only was obtained in the rectification of the crude oil from the sodium-salt.

The portion distilling between 280° and 300° C., when submitted to the action of sulphuric acid and acid chromate of potassium, yielded a crystalline cake, which, after washing, was dissolved in hot benzol; on cooling, a few oblique prismatic crystals of sulphobenzolene were obtained, and likewise a large quantity of the needle-formed crystals, probably held dissolved in the oil previously to its being oxidized. These I am at present examining.

Decomposition of Sulphobenzolate of Ammonium.

This salt, which melts at about 200° C., is decomposed with great facility, and at a comparatively low temperature, yielding large quantities of bisulphite of ammonium and benzol, with some undecomposed sulphobenzolate of ammonium, and likewise a very small quantity of a crystalline substance slightly soluble in cold water, the only residue in the retort being a little carbonaceous matter. On rectifying the benzol obtained in this decomposition, a small quantity of a heavy oil was obtained, having a high boiling-point, and which deposited crystals on cooling. When oxidized, sulphobenzolene seems to be formed in small quantity.

Sulphobenzolamide.

The crystalline substance which occurs in small quantity, amounting to about one and a half per cent., among the products of the destructive dis-

tillation of sulphobenzolate of ammonium, is washed with cold water to free it from the ammonium salts with which it is accompanied, dissolved in boiling water, and filtered to separate it from adhering traces of oil. On the cooling of this solution, the impure sulphobenzolamide is deposited in large micaceous scales. By one or two crystallizations out of spirit, and one from boiling water, it is obtained perfectly pure and white.

I. ·350 grm. gave ·590 grm. CO_2 and ·144 grm. H_2O.
II. ·442 grm. gave ·740 grm. CO_2 and ·182 grm. H_2O.
III. ·343 grm. gave ·219 grm. platinum.
IV. ·237 grm. gave ·148 grm. platinum.
V. ·252 grm. gave ·376 grm. sulphate of barium.
VI. ·432 grm. gave ·645 grm. sulphate of barium.
VII. ·124 grm. gave ·186 grm. sulphate of barium.

	Theory.	I.	II.	III.	IV.	V.	VI.	VII.	Mean.
$C_6=72$	45·85	45·98	45·66	..	..	..	..	..	45·82
$H_7=7$	4·46	4·57	4·57	..	..	..	..	..	4·57
$N=14$	8·92	..	..	9·03	8·84	..	..	..	8·93
$S=32$	20·38	..	..	..	..	20·46	20·48	20·58	20·51
$O_2=32$	20·38	..	..	..	..	..	..	..	20·17
157	99·99								100·00

These analyses correspond to the formula $C_6H_7NSO_2$, which is that of sulphobenzolamide, equivalent to sulphobenzolate of ammonium minus one atom of water, $\left.\begin{matrix}C_6H_5\\NH_4\end{matrix}\right\}SO_3-H_2O=C_6H_7NSO_2$.

Sulphobenzolamide crystallizes in large and very lustrous micaceous scales, greatly resembling naphthalin in appearance. It fuses at 153° C., and recrystallizes on cooling; when more highly heated, it volatilizes. It is extremely difficult to reduce the dry crystals to powder, owing to their toughness. When boiled with a strong solution of potash it gives off ammonia, sulphobenzolate of potassium being apparently formed at the same time. Weak acids have little or no action on it. The analyses were made for me by my assistant, Mr. C. E. Groves.

XII. "An Account of the Base-observations made at Kew Observatory with the Pendulums to be used in the Indian Trigonometrical Survey." By Balfour Stewart, F.R.S., and B. Loewy, Esq. Received June 15, 1865.

[This Paper will be published in a subsequent Number.]

XIII. "An Inquiry into the possibility of restoring the life of Warm-blooded Animals in certain cases where the Respiration, the Circulation, and the ordinary manifestations of Organic Motion are exhausted or have ceased." By BENJAMIN WARD RICHARDSON, M.A., M.D. Communicated by Dr. SHARPEY. Received June 14, 1865.

The present memoir presented to the Royal Society is preliminary; it does not profess to do more than to open the way to new work, and to show the reasons why the restoration of action, in cases where life is suspended, is at present so doubtful and difficult.

In the course of my inquiries I have not confined myself to the mere question of treatment as applied when there are still faint indications of spontaneous animal action, or when such action has ceased only for the moment. It is true that many of the experiments related in this Paper have reference, incidentally, to treatment under the circumstances named; but I have had actually in view a much wider research. I have asked, When an animal body that has undergone no structural injury (that is to say, no destruction of organ or tissue) has ceased to exhibit those actions which indicate what is commonly called life, why may it not be restored at a period previous to the coagulation of the blood in its vessels, if not previously to the period when the new chemical changes, developed under the form of putrefaction, are established?

To render the memoir concise, I have divided it into two parts. One of these parts contains nothing more than the details of experiments, the experiments being classified in groups according to the object for which they were performed. It is my desire that this record should be preserved for reference in the Archives of the Society. The other part, which is here published, consists of an analysis of the experimental evidence, with the conclusions to which I have been led by the evidence.

ANALYSIS OF THE EXPERIMENTAL EVIDENCE.

In the experimental inquiry all the animals operated upon had been subjected to such means for suspending their animation as produced the least possible amount of change in the structure of organs. The animals were all healthy while living. To suspend the spontaneous action which they presented, and which marked their life, chloroform was employed in the large majority of cases; but in some instances carbonic acid was used, and in others the process of drowning.

The readiness with which chloroform can be employed, and the painlessness to the subject which is implied in its use, recommended this agent specially at first. As the inquiry has proceeded I have seen no reason, so far, to introduce any modification, inasmuch as the continuance of experiment and repeated observation have simply tended to indicate that the process called "death" is unity; and that if animal action, brought to a stand by chloroform, could be reproduced by any process, the same restorative

process would be applicable after every other kind of suspension that was unattended by mechanical injury of structure.

Throughout the inquiry I have kept steadily in view a process for restoring the development of force which is constantly and successfully being performed. A simple process enough! I mean the relighting of a taper. I see in the taper as it is burning the analogue of living action. The combustible substance having the force stored up in it circulating through the wick as through so many vessels, becoming distributed in the presence of incandescent heat so as to combine with oxygen; then itself liberating force, burning, and in the process showing spontaneous action, the analogue of living action.

From this analogy I gather, further, that if I could set the blood burning as it burns in life, after its natural combustion has been suspended, I should relight the animal lamp, and that the redevelopment of force in the form of animal motion, which is life, would be reestablished.

But how in the case of the animal body is the light to be applied? That is the difficulty.

Suppose that the taper or the fire were known only to us from their spontaneous manifestations, would the task to restore their burning if that had gone out be less difficult? What philosophical process should we adopt? We should first most naturally take fire from fire when that were possible. But how, when that were not possible, should we proceed to obtain the spark for kindling that which we might well know would burn spontaneously after kindling, the proper conditions being supplied? In such case we should most naturally look for the process by which fire is spontaneously exhibited, and we should discover it in the friction of one body with another; in the friction of stone, for example, with iron. Straightway we should imitate this and produce fire, and know how to renew and perpetuate it.

Again, in our observation of burning bodies we should see often that a point of flame well-nigh extinguished would rekindle under a little additional friction of air, or an additional communication of matter that would burn, and we should acquire an art of sustaining fire by these measures.

Lastly, as we went on observing we should discover that the force elicited in the combustion could be so applied as to set in motion almost endless mechanism; and we should learn, as we have learned, that however complicate the mechanism, however numerous its parts, it takes all its motion from the fire.

The physiologist who would distinguish himself by learning the art of resuscitation must, I have thought, place himself precisely in the same condition as the primitive man who, in the matter of ordinary combustion, would pass to the civilized man through the phases I have described; and it seems to me that, so far as we have progressed we have become acquainted with three natural steps in the inquiry at least. We have discovered that when the animal fire is declining from want of air, it may be fanned into

existence again by gentle friction of air. We have learned by an experiment, first thoroughly demonstrated before the Royal Society in the early days of its remarkable history, that when the animal fire is waning, owing to deficiency of fuel, that is to say, of blood, it may be revivified by the direct introduction of new blood. Lastly, we have learned that the natural or spontaneous combustion of blood is due to the affinity of the oxygen of the air for combustible substance in the blood, when such substance is presented to the air over a sufficient extent of surface.

These observations may be received as demonstrable truths; and to them may be added an inference which amounts nearly to a demonstration, though all its elements have not yet been estimated—that the motion of the animal (the action of its mechanical parts) is produced by the force evolved in the process of combustion.

The experiments submitted in this paper have reference to the best means to be adopted for fanning into active life the animal fire that is expiring but is not suspended. But they extend also to the deeper questions, whether animal combustion cannot be reestablished when it appears to have been extinguished? and whether so-called vital acts would not be spontaneously manifested upon such reestablishment of animal combustion?

In the part of this paper which contains the details of experimental research, the experiments are classified in three series.

The first series of experiments has reference to attempts made to produce combustion of blood in the lungs by the introduction of air—*Artificial Respiration.*

The second series embraces experiments in which attempts were made to induce circulation of the blood by physical operations—*Artificial Circulation.*

The third series supplies the records of experiments in which the effects of *an increased temperature* upon the body were observed.

First Series of Experiments.—*Artificial Respiration.*

Effects of simple inflation of the lungs with air.—The first series of experiments, those in which artificial respiration was employed, exhibits, I believe faithfully, the precise value of artificial respiration. In the preliminary inquiries, the animals, having ceased to breathe, were immediately subjected to artificial inflation by means of double-acting bellows. The result in every case was, that whenever the action of the heart had come to rest, the temperature of the air employed being at various degrees, from 40° to even 120° Fahr., no reaction followed the inflation.

In opening the bodies of animals thus treated, the lungs were invariably found empty of blood, and in a large number of cases emphysematous, while the right side of the heart was filled with fluid blood.

The heart continues to beat when artificial respiration restores.—In one striking experiment, where respiration had entirely ceased and no action of the heart could be detected from pulsation, a recovery took place in a dog.

Narcotism was again carried on to the same extremity, with recovery on inflation; and this was repeated once more with the same result. But in this experiment, although, to appearance, animal action was entirely suspended, a minute examination of the heart, through an opening in the skin sufficiently large to allow the mouth of the stethoscope to rest on the ribs, but not to injure either them or the intercostal muscles, proved that there was still sufficient action of the heart to produce a faint first or systolic sound.

Cessation of the heart during artificial respiration. Order of cessation.—These experiments by inflation were modified. So soon as the animal ceased to exhibit evidence of life, the artificial respiration was set up, the chest-wall was removed, and the effects of the artificial respiration on the heart were observed. In every case where the operation was performed within five minutes the heart was discovered pulsating. The action was uniformly best marked in the right auricle, next in the right ventricle, next in the left auricle, and next in the left ventricle. Contraction remained longest also in the same order. But it was observed uniformly that the contraction of the right ventricle never sufficed to fill the pulmonary artery with blood so as to reestablish the pulmonic circuit.

Effect of inverting the body.—In one case the animal was suspended with the head downwards while the right ventricle was contracting vigorously. In this case blood passed into the pulmonary artery and faintly coloured the surface of the lung, which was previously pale; but the pulmonic circuit was not reestablished, and after death the capillaries were found to be obstructed mechanically from coalescence of the blood-corpuscles.

Effects of artificial inflation with air raised in temperature.—These experiments with the chest laid open were varied by the employment of air at different temperatures. The evidence was clear that when the contractions of the heart were failing, an increase in the temperature of the air to 140° Fahr. caused a more vigorous action, which often lasted from five to ten minutes.

Effect of exposure to the air in exciting cardiac action.—To determine whether the act of insufflation of air at a mean temperature of 60° was sufficient of itself to set up contraction of the heart, two animals were destroyed with chloroform and allowed to rest fifteen minutes. Then in one animal artificial respiration with air at 60° was employed for five minutes, and the hearts of both animals were immediately exposed to view. There was no action in either case at first; but after exposure to the air for a few minutes the right auricle in both hearts commenced to contract, and the ventricles followed. But the action was in no way more determinate in the animal that was receiving air by inflation than in the other animal. I notice this point particularly, because some experimentalists, who have made but one or two observations, on seeing the heart pulsate during artificial respiration, have conceived that the phenomenon was due solely to the in-

flation. I believe it myself to be due to the action of the external air, which at a moderate temperature gives up a little oxygen to the blood in the walls of the heart, by which some heat is evolved and therewith motion is exhibited. My reasons for this view rest on the facts that a current of air at 35° Fahr., brought to bear on the heart, at once stops the action, while another current above 60° restores it, and that a little vapour of chloroform or of ammonia blown upon the heart—both of which agents stop oxidation—immediately arrests the action, which returns, at a sufficient temperature, when these agents are lost by diffusion. I believe also that the right auricle is last to die, because its thin walls allow the passage of oxygen to venous blood on their interior, since on washing out the auricle thoroughly with water, or on applying to it a substance which prevents oxidation, the auricular motion at once declines.

These remarks on the effect of artificial respiration in relation to the motion of the heart, do not apply with the same force when the air employed for inflation is heated to 120° Fahr.; then even fifteen minutes after death, if the inflation be sustained, the heart is found contracting as the chest is laid open, the action really being sustained by the diffusion of heat from the lungs to the heart; but the action excited is insufficient to produce a pulmonic current.

Effects of other gases used in artificial respiration.—The experiments were further modified by using for insufflation other gases in place of common air. Oxygen was thus used, oxyhydrogen, ozone, and air containing 0·20 per cent. of chlorine. With two exceptions, the same observations are applicable to these experiments as were made in reference to those with common air. As a rule, the gases possessed no action on the heart to restore the pulmonic current when the natural action had been arrested. The exceptions were, that when the action of the heart was still feebly proceeding, respiration not being suspended, the air containing chlorine or ozone produced a quicker restoration, the ozone being much the less objectionable in regard to its after-effects.

Artificial respiration by electro-galvanic action.—The experiments on artificial respiration were finally modified by using the electro-galvanic current to excite the muscles of respiration so soon as natural respiration and circulation had ceased. By inserting a fine needle, insulated except at the point, into the larynx of an animal, and the other needle into the diaphragm, and by regulating the shock by means of a metronome, so that a given number of shocks representing the respirations of the animal are administered, the most perfect appearance of natural respiration may be sustained for so long, in some cases, as seven minutes; and the phenomena are often remarkable, and to the inexperienced deceptive. Thus, owing to the action on the vocal apparatus, a rabbit will scream as loudly as in life; and, lying breathing and screaming, might well be considered to be alive. But all the while the heart is at rest, if it have once rested, and on opening the chest the lungs are found bloodless.

Résumé.—Value of Artificial Respiration.

Reviewing the whole series of experiments, I am led to the conclusion, and I think it admits of direct demonstration, that artificial respiration, in whatever way performed, is quite useless from the moment when the right side of the heart fails in propelling a current of blood over the pulmonic circuit, and when the auriculo-ventricular valve loses its tension on contraction of the ventricle.

Break of blood-column.—At this point the blood-column is broken: the resistance to the passage of blood is of itself almost overwhelming, while the muscular action is decreasing in power in proportion as the difficulty of propulsion is increasing.

Obstacle from coalescence of blood-corpuscles.—Another obstacle is in the blood itself. It consists in the rapid coalescence of the blood-corpuscles as the motion of the blood ceases. This is so determinate, that within three minutes after its complete cessation, the blood, though still fluid, often fails to be carried, even by a moderately strong stroke, over the lungs. In one experiment the chest of a strong dog was laid open while the animal was under chloroform, and artificial respiration was sustained. Both sides of the heart were acting vigorously, and there was a good arterial current. In the midst of this action, which could easily have been sustained for an hour, the pulmonary artery was suppressed for the space of two minutes and fifty seconds. Then it was liberated, and the ventricle, which was still beating vigorously and gave out a valvular sound, carried the pent-up column into the pulmonary vessel; but there was no circuit. The lung was somewhat congested, and the capillaries were blocked up so as to resist an impulse which, increased by galvanism, was more active for some minutes after the liberation of the artery than it had been previously.

Obstruction from coagulation of blood.—The last obstruction is the coagulation of the blood; but as this does not, as a general rule, occur (in cases where the blood-vessels are not opened) within twenty minutes, and often not within an hour, it may be considered a secondary difficulty, though naturally fatal to success, according to our present knowledge, when it has taken place.

Modes of applying Artificial Respiration.

Regarding the modes of applying artificial respiration, and the time, the facts are briefly as follows:—

1. It is unnecessary and even injurious to employ it so long as there is any attempt at natural respiration*.

2. Before employing it, the patient should be placed with the head slightly lowered, a position which will largely assist the right ventricle in any feeble effort it may be making to propel a current of blood into the pulmonic circuit.

3. It is of the greatest importance that the air conveyed into the lungs

* On this point see observations 20 and 21 in the Experimental Part.

should be at a temperature above 60°; air below that temperature should never be used.

4. All violent attempts to introduce large quantities of air are injurious; for whenever the pressure of the blood from the right side of the heart is reduced, the danger of rupturing the air-vesicles by pressure of air is increased. In a word, the practitioner should remember that he is doing the same act, virtually, in artificial respiration, as he is when attempting to relight an expiring taper. Any violence will only disarrange the mechanism, and turn the last chance of success into certain failure.

5. So long as care be taken to sustain a gentle action of respiration, it signifies little, in my opinion, what means be employed. I have found a double-acting bellows, described in the experimental part of this paper, answer every purpose fairly. If any philosophical-instrument maker could invent a good and portable electro-magnetic machine with my metronome principle applied to it, so that from fifteen to twenty shocks per minute could be passed from the larynx to the diaphragm directly, the most perfect attainable artificial respiration would be secured so long as any muscular irritability remained; and I should suggest the value of such an instrument in cases where it could be brought into operation immediately after natural respiration has ceased. In combination with air heated from 120° to 140° for inhalation, every possible advantage that could accrue from artificial respiration, or rather from respiration artificially excited, would be secured, the persistence of muscular irritability being at the same time a sure index that the effort should not cease.

Note on Receiving-houses for the Drowned.

The observations I have made in respect to the influence of heated air lead me to suggest that, in all receiving-houses for those who are apparently dead, a room should be set apart the air of which should be at 140° in summer, and 130° in winter. If bodies taken out of the water showed any indications of breathing, it would be sufficient, in my opinion, to place them in such an atmosphere, simply providing by the position of the body for the escape of water from the lungs. There would be under such conditions quick evaporation of water adhering to the bronchial surface, while the warm air would quicken the respiration, encourage the action of the heart, and prevent radiation of heat from the body. If artificial respiration were considered necessary, its performance in such an atmosphere would render the possibility of recovery far greater than if a low temperature and a moist state of atmosphere prevailed.

Second Series of Experiments.—*Artificial Circulation.*

In the second series of experiments an attempt was made by various physical means to restore the circulation; these attempts may be called attempts at *artificial circulation.* Various processes were adopted. In one class of inquiry oxygen was gently infused into the circulation, either in the form of gas, or in solution as peroxide of hydrogen, in order to see

if by this means the heart could be stimulated to active contraction. In other instances water heated to a given temperature was injected, or the vapour of water. Again, electricity was brought into play; and, lastly, various mechanical contrivances were introduced, either for forcing the blood over the system or for drawing it over.

Injection of oxygen.—In respect to the effect of oxygen gas, I found that when the gas freshly made from chlorate of potassa, but well washed, was driven into the venous current towards the heart by the vena cava superior, the auricle and ventricle of the right side at once exhibited active contraction, which could be prolonged for an hour without difficulty by simply continuing the introduction of the gas at intervals; but the contraction of the ventricle was never sufficient to produce a pulmonic current. When the gas was injected into the arteries, the current being directed towards the heart, so as to charge the structure of the heart itself with the gas through the coronary arteries, the heart in one instance made active movements which could be distinctly felt through the chest wall; but the effect was only momentary; and after it was over, the organ was found distended with gas and devoid of irritability. In another case, on making a *post-mortem* examination of an infant that had been dead twelve hours, oxygen gas at a temperature of 96° was injected into the heart. The organ became gradually distended; and on the left side, both in the auricle and in the ventricle, tremulous muscular action, like very feeble contraction, was distinctly seen. Whether this was due to the mechanical entrance of the gas or to true muscular contraction excited by the presence of the gas, is perhaps open to question, but I could make no distinction between this contraction and ordinary contraction as it is elicited immediately after death. The subject of this experiment was fourteen days old. Previous to the injection there had been no cadaveric rigidity, but after the injection this phenomenon was well marked.

Injection of peroxide of hydrogen.—The experiments with the peroxide of hydrogen were varied by passing the solution very slowly into the lung through the trachea, so that the oxygen that would be liberated might come into contact, together with the air afterwards introduced by the bellows, with any blood remaining in the pulmonic circuit. A little fluid during this process found its way into the left auricle through the pulmonary veins, and the auricle thereupon contracted. On injecting the peroxide, in another experiment, over the arterial system, the blood on the venous side was pushed forwards into the heart and it was made red in colour from absorption of the oxygen. As the fluid found its way round the systemic circuit, vigorous muscular contraction of the pectorals, of the muscles of the neck, and of the diaphragm followed, but there was no reaction of the heart.

Oxygen excites muscular action.—I gather from these researches that oxygen, introduced into the circulation directly, possesses the power of calling forth muscular contraction. This power seems to be due to the combination of the oxygen with a little blood remaining in the circulatory

channels, and to the evolution of force from that combination. The effect of the oxygen, therefore, is extremely limited; and when introduced in the gaseous form, the distention it produces leads to a certain degree of disorganization of structure. I do not at this moment see therefore that oxygen admits of being applied as a direct excitant of the heart; but it is worthy of remembrance that the element produces temporary excitability when diffused through muscular structure recently rendered inactive.

Heat as a restorer of the circulatory power.—A large number of attempts were made to restore the circulation by means of heat conveyed into the vessels by heated fluids. The phenomena produced were very remarkable, and they have engaged my attention for more than five years. I first observed that when vapour of water (steam) at a temperature of 130° was driven into the arteries, there was at once rapid and general muscular action, the heart participating in the movement, but less actively than the voluntary muscles.

Injection of heated water.—I afterwards used simple water for injection heated to various degrees, from 96° to 130°. When water is thus injected, the animal being only a few minutes dead, and the water not being below 115° Fahr., the extent and activity of the muscular contractions are even more marked than when galvanism is brought into action, but in the greater number of cases the effect of the warm water ceases in from fifteen to twenty minutes. When the temperature of the air in which the animal lies is below 40°, the water will act for so long a period as three hours after death. The water ceases to exert its influence when it infiltrates the cellular tissue. The admixture of salt with the water, so as to raise the specific gravity to the natural specific gravity of the blood, unquestionably diminishes the effect of the heated water; the muscular contractions are less rapid and less prolonged, although the infiltration into the cellular tissue is prevented for a much more lengthened period of time. I attribute the action produced on the muscles entirely to the heat evolved by the water.

*Injection of blood**.—Injection of blood held fluid by alkali, oxidized and heated to 96°, was employed. The blood was injected into the carotid in the direction of the heart, the object being to fill the coronary arteries with the fluid. This intention was fully carried out; but although the animal had been only a few minutes dead, there was no response on the part of the heart.

In another experiment, blood from the sheep, defibrinated, thoroughly oxidized, and warmed to 115° Fahr., was injected into the arterial system immediately after the death of an animal (a rabbit that had been destroyed by chloroform). The right auricle having been opened to allow of the escape of venous blood, no difficulty was experienced in forcing over the oxidized blood, and it returned freely by the veins; but it did not excite the least contraction. When this transfusion had been carried on some minutes, the blood was replaced by water at 125° Fahr. Immediately as the water found its way round the body, vigorous action of the body was

* See note, p. 369.

manifested, with facial movements extremely like life; and these movements, by repeating the injection, were sustained for an hour. This experiment shows that heat alone was the restorer of the muscular irritability.

Electricity as an excitant of the circulation.—Electricity, in the form of electro-galvanism, was employed in several experiments and in various ways to excite the heart. The little battery of Legendre and Morin, with the addition of the metronome so as to regulate the stroke, was the instrument used, and artificial respiration was combined with the electric process. In one experiment the negative pole from the battery was passed along the inferior cava into the right side of the heart, and the opposite pole, armed with sponge at its extremity, was placed over the heart externally. Sufficient action was excited to produce a pulmonic current by the contraction of the right ventricle. The left side of the heart also contracted on receiving blood, an arterial circuit was made, and the animal exhibited for the moment all the signs of reanimation. In another case the insulated pole from the battery was passed into the left side of the heart of a dog, the opposite pole being placed on the divided chest-wall. There was immediate action of all the muscles of the chest, but the heart was uninfluenced. In a third case a current was passed from the brain along the whole length of the spinal column, and artificial respiration was sustained for half an hour. On opening the body, the heart was found full of blood on both sides and was contracting, but not with sufficient force to produce a circuit.

In a fourth case, a dog being the subject of experiment, electric communication between the right side of the heart and the external part of the organ was set up with artificial respiration, as in the first experiment of this kind, only that the poles were reversed, and at the beginning of the experiment the pole applied ultimately to the heart externally was placed for a few minutes previously over the intercostal muscles. In this experiment the heart did not respond at all, although the thoracic muscles made vigorous contractions.

The inference which I draw from these experiments with electricity on the heart is, that by rapidly establishing a direct circuit between the blood in the right side of the heart and the external surface of the organ, using a moist conductor from the positive pole for the external surface, a sufficient contraction may (I had almost said, by a fortunate accident) be induced in the right ventricle to drive over the pulmonic current of blood, and to allow of its oxygenation by artificial inflation of the lungs. This fact at first sight looks small; but I value it beyond measure, because it has demonstrated that, when the action of the heart has ceased, the chest of the animal being open and all the conditions for reanimation being most unfavourable, the mere passage of blood from the right to the left side of the heart is sufficient to reestablish the action of the left side; that the left side thus reacting can throw a blood-current into the arteries; and that upon the reception of blood by the system, general muscular action and rhythmical action of the muscles of the chest are reproduced.

Advantages and dangers of galvanism.—In considering the advantages that may be derived from galvanism, certain dangers of it must not be forgotten. My experiments clearly showed that the natural muscular irritability, while it is for a short time made more active by galvanism, is shortened in duration. This is natural. The irritability of muscle is in proportion to the degree of force which remains in it after the blood is withdrawn, which force is evolved in proportion as it is called forth. It is well, therefore, in applying galvanism for any purpose to the subject in whom the action of life is suspended, to use the agent for one definite object, and to remember that, in proportion as it is used, its power for good diminishes.

Mechanical methods for restoring the circulation.—In the last division of the physical series of experiments, the object held in view was to set the blood mechanically in motion through its own vessels. The attempts were made (*a*) by forcing blood towards the right side of the heart and into the lungs by the action of a syringe fixed in a vein, (*b*) by trying to draw over a current of blood into the arteries from the veins and over the lungs, (*c*) by trying to inject the heart of one animal with blood derived from another animal.

Forcing-action by the veins.—*A priori* it seems an easy task to take an animal so soon as it is dead, to fix a tube from a syringe in the external jugular vein, to fill the syringe with blood, and by a downward stroke to push on the blood in the course of the circulation. From a mechanical point of view, the operation is perfect in theory; and when we remember that the auriculo-ventricular valve of the right side becomes in fact a natural valve for the piston, it is difficult to see how an artificial circulation can fail to be established by this simple means. When we further remember how easy it is to combine artificial respiration with the propelling process, one must feel that, prior to a point of time when the blood has coagulated, the process ought to succeed. Indeed, when the suggestion first occurred to me, I was so struck by it, that I rose from bed in the middle of the night to carry it out. Without for a moment losing faith in it, it has not as yet, however, been successful in my hands. The practical difficulty lies in the adjustment of the force employed. If too much force be used, the vein gives way; if too little, the obstruction in the pulmonary artery and lungs is not overcome. In further researches I shall employ larger animals than I have done up to the present time.

Suction-action by the arteries.—While conducting this forcing-process for artificial circulation, another idea suggested itself, viz., that perhaps it would be possible to *draw* a current of blood over the lungs into the arteries, oxidizing the current as it passed by artificial respiration. With this object in view, a syringe, connected with an air-pump, was fixed in a large artery, and the barrel of the syringe was then exhausted. When the syringe was thus filled with blood, the motion of its own piston downwards pushed the blood back into the arteries in the direction of the heart. The

difficulties in this experiment were connected with the rapid coagulation of blood; but here, as in a previous experiment, sufficient was indicated to prove that reanimation is a possible fact. In one case the syringe was filled with blood, brought over the lungs and oxidized; and when this blood was driven again over the arterial circuit into the muscles, it reestablished, wherever it found its way, muscular action, and, for a brief period, all the external phenomena of life.

Transference of motion from living to dead hearts.—Equally interesting with the results just named were those in which it was attempted to stimulate to contraction the dead heart of one animal with the force derived from the blood issuing from the heart of a living animal. In the experiment related as bearing on this point, although the force could not be readily conveyed by the pulsating stroke of the living heart, it was shown that twenty-eight minutes after the dead heart had ceased to pulsate, its contractions were revived by the transference of the blood derived from the heart of the animal that lived.

Artificial blood for injection.—It remains to be seen whether a fluid resembling arterial blood, and capable either of being readily compounded when required, or of being kept ready for use, and capable also, when heated to 98°, of restoring the muscular power of the heart, may not be invented. If it can, then the operation of injecting the heart by a carotid or brachial artery will be the most important practical step yet made towards the process of resuscitation when the motion of the heart has been arrested.

Value of the heart-stroke.—Granting, however, that such a fluid could be discovered, it would be necessary, in using it, to feed the heart, not in one continuous stream, but stroke by stroke, as in life; for it seems to me that the stroke supplements or, more correctly speaking, represents a certain measure and regulation of the force derived from the combustion of the blood. After many failures, I believe I have at last contrived an injecting-apparatus which will supply the stroke at any tension and at any speed that may be required; but the instrument is not yet out of the maker's hands.

Bearing on this subject, it is certain that blood at 98° in the living heart will excite spontaneous action of involuntary muscle; that blood which has been drawn, oxidized, and heated even to 115° will not excite spontaneous muscular action when injected in a continuous stream, but that water or blood at 125° injected with a continuous stroke will excite. It is essential, therefore, to determine whether the addition of mechanical force by stroke will supplement the necessity of a higher temperature*.

* Since this paper was laid before the Society, I have determined by a direct experiment that rhythmic stroke is of the first importance in restoring muscular contraction. By means of a machine which can either be worked by the hand or by electro-magnetism, I was enabled, assisted by my friends Drs. Wood and Sedgewick, to introduce blood heated to 90° Fahr. into the coronary arteries of a dog by rhythmic stroke, and at the

Third Series of Experiments.—*Application of External Heat.*

The last series of experiments were conducted to ascertain the effects of external heat applied to the body that has ceased to show evidence of life. I was led to the inquiry by the fact that a kitten that had been under water, to my direct knowledge, for two hours, became reanimated in my pocket, and lived again perfectly. To see what further could be done in this direction, I placed three young rabbits, which had been drowned, in a sand-bath at temperatures respectively of 100°–110° and 120°. Afterwards other rabbits that were destroyed by carbonic acid and chloroform were placed in the same manner and exposed to the raised temperature for an hour. In no case was there any restoration of vitality; but it was observed that those parts of the body that had been more directly exposed to the heat showed the earliest indications of cadaveric rigidity. In the experiments where the death took place from chloroform, and where the animals had been exposed to a temperature of 100°, the heart at the end of an hour was found still excitable, and on the right side was contracting well without the application of stimulus. This did not occur in the cases of death from drowning and carbonic acid, nor yet in cases where the warmth was carried above the natural temperature. These observations are of moment as indicating two facts,—viz., that chloroform is less fatal as a destroyer of muscular irritability than either carbonic acid or the process of drowning; and that in the application of temperature to the external surface of the body by the bath, it is not advisable to raise the temperature many degrees above the natural standard.

It is worthy of remark that in one of the rabbits which had been destroyed by chloroform and exposed to a temperature of 100°, the muscular irritability in the intercostal muscles was present thirteen hours after death. In all the cases the right side of the heart was found free of engorgement, while the left side and the arteries contained blood—thus indicating that a pulmonic current had been produced.

General Conclusions and Indications.

I have already shown that artificial respiration is of service only when blood from the heart is being still distributed over the capillary surface of the lungs—or, to return to the simile with which I set out, that the process is simply one of fanning an expiring flame, which once expired will not, in spite of any amount of fanning, relight. The further conclusion to which I am at this moment led, goes, however, beyond the process of artificial respiration; returning again to the simile, I venture to report that, even

same rate as the stroke of the heart of the animal previously to its death. The result was, that one hour and five minutes after the complete death of the animal, its heart, perfectly still, cold, and partly rigid, relaxed, and exhibited for twenty minutes active muscular motion, auricular and ventricular. The action, which continued for a short time after the rhythmic injection was withheld, was renewed several times by simply reestablishing the injection.

when the heart has ceased to supply blood to the pulmonic capillaries, during the period previous to coagulation, the blood may be driven or drawn over the pulmonic circuit, may be oxidized in its course, may reach the left side of the heart, may be distributed over the arteries, and that, thus distributed, it possesses the power of restoring general muscular irritability and the external manifestations of life. Hence I infer that resuscitation, under the limitations named, is a possible process, and that it demands only the elements of time, experiment, and patience for its development into a demonstrable fact of modern science.

Various modifications of the experiments to which I have had the honour to draw the attention of the Society are in hand; and if I am allowed the privilege, they will form the subject of another communication.

XIV. "On the Anatomy and Physiology of the Nematoids, parasitic and free; with observations on their Zoological Position and Affinities to the Echinoderms." By Henry Charlton Bastian, M.A., M.B. (Lond.), F.L.S. Communicated by Dr. Sharpey. Received June 13, 1865.

(Abstract.)

After commenting upon the many conflicting statements which have been made concerning the anatomy of these animals, and more especially with regard to the presence or absence of a nervous system, and of real organs of circulation, the author alludes to the increased interest which has lately been thrown over this order by the discovery of so many new species of the non-parasitic forms, marine, land, and freshwater.

He has entered fully into the description of the tegumentary organs, and has recognized a distinct cellulo-granular layer intervening between the great longitudinal muscles and the external chitinous portion of the integument. This layer is one of great importance in the economy of these animals; the author looks upon it as the deep formative portion of the integument from which the chitinous lamellæ are successively excreted. It is bounded internally by a fibrous membrane, which serves as an aponeurosis for the attachment of the four great longitudinal muscles; and the well-known lateral and median lines which have so long been a puzzle to anatomists are, he says, in reality nothing more than inter-muscular developments of this layer. In some species each of the lateral lines contains an axial vessel, though in very many others nothing of this kind is to be met with. A periodical ecdysis of the chitinous portion of the integument takes place in all Nematoids during the period of their growth.

The author agrees with Dr. Schneider as to the nature of the transverse fibres attached to the median lines. They are contractile prolongations from the longitudinal muscles, and may be considered extrinsic muscles for the propulsion of the intestinal contents, since the intestine itself has no muscular tissue in its walls.

Schneider's description of the nervous system in *Ascaris megalocephala* has been confirmed, and a similar arrangement has been recognized by the author in several other Nematoids. It exists as a nervous ring encircling the commencement of the œsophagus, in connection with many large ganglion-cells. The principal peripheral branches are given off from the anterior part of the ring, and proceed to the region of the mouth and cephalic papillæ. Although well developed ocelli exist in many of the free marine species, no nerve-filaments have yet been detected in connexion with them.

The organs of digestion are mostly simple, the principal variations being met with in the presence or absence of a pharyngeal cavity, and in the structure of the œsophagus. In some species its parietes are distinctly muscular, whilst in others, as in the *Trichocephali* and *Trichosomata,* they are as distinctly cellular. Those possessing a pharyngeal cavity sometimes have well-marked tooth-like processes developed from its walls; but the author believes that the chitinous plates which are sometimes met with in posterior swellings of the œsophagus are not "gastric teeth," as they have been hitherto described, but rather valvular plates for ensuring greater perfection in the suctorial process by which these animals pass their food along this portion of the alimentary canal.

The water-vascular system may be seen in many Nematoids in its most elementary condition, as a small tubular gland, with an excretory orifice in the mid-ventral region of the anterior part of the body. In other Nematoids no trace of such a system exists; whilst its most developed condition yet recognized in these animals may be seen in *Ascaris osculata* and *A. spiculigera,* where an intimate plexus of vessels, still in connexion with an anterior ventral pore, is met with in a peculiar development from the left lateral band. Intermediate conditions between these extreme forms may be traced in other species; and from the obviously glandular nature of the tubular or pyriform organ met with so commonly in the free, and also in many of the parasitic species, he thinks considerable light is thrown upon the function of the "water-vascular" system. He says, "Here we have undoubtedly to deal with an excretory glandular apparatus. No one could for a moment regard these structures as at all analogous to vessels destined alternately to receive and discharge an external fluid medium. I believe that in the *Trematoda* and *Tæniada* also, where similar though often more developed systems exist, their function is in like manner one of a purely eliminatory kind; and I therefore cannot but look upon the name of 'water-vascular' apparatus as a singularly inappropriate appellation for this system of vessels."

Other very peculiar transverse vessels exist in the deep integumental layer of *Ascaris megalocephala* and *A. lumbricoides,* mostly running in pairs from median line to median line, and, strangely enough, being about twice as numerous on the right as on the left side of the body.

The author believes that in the Nematoids but little provision exists for

the oxidating portion of the process of respiration, and thinks that this deficiency may be compensated by a greatly increased activity of glandular *eliminating* organs. Considering the conditions under whose influence so many of the parasitic forms pass their existence, we can easily imagine that the presence of any organs for effecting an oxidation of their tissues would not only be useless, but actually baneful. Glandular organs exist in the greatest abundance in all Nematoids, and many of these are excretory organs. In those species in which no modification of the ventral excretory apparatus is met with, the author has found a very large number of channels running through the chitinous portion of the integument, so as to bring its deep cellular layer in communication with the exterior. These pores are, he believes, complementary respiratory organs, and their development is always in an inverse proportion to that of the other excretory organs. Thus amongst the free Nematoids he has found them most numerous in *Dorylaimus stagnalis* and *Leptosomatum figuratum*—species in which the ventral excretory apparatus is entirely absent. The same arrangement is met with in the *Trichocephali* and *Trichosomata,* in which these integumental channels attain their maximum development. The gradually widening longitudinal band long known to exist in the *Trichocephali* is due to the presence of thousands of these channels in connexion with a glandular development of the deep integumental layer beneath.

Many interesting facts are brought forward concerning the "tenacity of life" of some of the free Nematoids, and their power of recovery after prolonged periods of desiccation. This has been long known as one of the characteristics of *Tylenchus tritici**, but the author has found it common only to the species of four land and freshwater genera,—*Tylenchus, Plectus, Aphelenchus,* and *Cephalobus.* The remainder of the free Nematoids are remarkably frail, and incapable of recovering even after the shortest periods of desiccation.

In the last section, on "The zoological position and affinities of the Nematoids," the author enters fully into what he believes to be the points of resemblance between these animals and the Echinoderms. The strongest evidence is, he thinks, to be found in the fact of the very close resemblance between the nervous systems of these animals, differing notably as it does at the same time from what we find in the *Scolecida* or *Annelida.* Then the integumental pores which he has now discovered in so many Nematoids can, he thinks, be paralleled only by the ambulacral and other pores met with in the Echinoderms. Great similarities in the distribution of these pores may also be observed in the two groups. The Nematoids present no trace of segmentation or lateral appendages to their bodies, but traces of a radiate structure do exist. Their various parts and organs exhibit a quadrate mixed with a ternate type of development. He looks upon the order *Nematoidea* as as aberrant division of the class *Echinodermata,* which at the same time tends to connect this class in the most interesting

* *Vibrio tritici* of older writers.

manner with the *Scolecida*—since, although in the points above mentioned they display their affinities to the Echinoderms, still, as regards the structure and different modifications of the ventral excretory apparatus, they agree more closely with the *Trematoda* or flukes.

XV. "On the Development of Striated Muscular Fibre." By Wilson Fox, M.D., Professor of Pathological Anatomy in University College, London. Communicated by Dr. Sharpey. Received June 15, 1865.

(Abstract.)

The discrepancies in the statements made by various observers on the structure, as illustrated by the history of the development, of striated muscular fibre, have induced the author to submit the question to a renewed and independent investigation. He has examined the process in the tadpole, the chick, the sheep, and in man, and with results which correspond very closely in all these classes. The investigation is most easy in the tadpole, as the early structures are of much larger size; but observations are made with a comparatively greater precision when high magnifying powers are employed. The author has used 900 linear in his observations on the tadpole, 1250 or 1850 linear in his observations on the chick and mammalia. The earliest form in which muscular tissue appears in the tadpole is an oval body containing one or more nuclei, and densely filled with pigmentary matter. This body has a well-defined outline, which induces the author to regard it as a cell, though he has not succeeded in isolating any distinct membrane. Such bodies then increase in length with or without multiplication of their nuclei, and after a short period a portion of their structure loses in great part its pigment and exhibits a striation sometimes transverse, sometimes longitudinal, or occasionally both conjointly; but there is no distinct line of demarcation at this stage between the striated and non-striated portion of the cell-contents,—showing that the change takes place within the contents of the cell.

As the pigment gradually diminishes in the non-striated portion of the cell-contents, a membrane can in some cases be very distinctly observed limiting the whole structure, while in others it can only be seen around the non-striated portion, and in the former case the presence of a striated structure within this membrane is very distinct. The nuclei are always found situated in the granular non-striated portion of the contents of the cell.

The cell may elongate to a very long fibre, to which only a single nucleus may be attached, or in the process of elongation a great increase in the number of nuclei may take place. In all cases the nucleus and fibre are enclosed by a membrane, which the author regards as an extension of the original membrane enclosing the cell in its earlier stages. The thickness of the striated portion appears to be in direct proportion to the number of nuclei enclosed within the membrane.

With the advance of development the space occupied within the mem-

brane by the granular non-striated as compared with the striated portion of the fibre diminishes, so that the latter almost entirely fills the membrane, the nuclei lying within the membrane but external to the striated portion, and surrounded by a small amount of the granular matter of the original cell-contents.

The differentiation of the muscular fibre of the chick commences in the dorsal region, according to the author's observations, after about forty-eight hours of incubation. Here the first appearance is of nucleated oval bodies with well-defined outlines, but much smaller than in the tadpole, which the author regards as cells, though he has been unable to isolate a membrane. These rapidly elongate into fusiform bodies, in which sometimes a faint striation becomes apparent. Shortly after the commencement of the third day long fibres appear, apparently from the elongation of the former, which are striated both longitudinally and transversely, and upon them is situated a nucleus, around which is some granular matter (the remains of the original cell-contents), the whole being enclosed by a membrane.

From the fourth to the fifth day a great multiplication of the nuclei follows within the membrane, and in proportion to this multiplication does the diameter of the fibre, and also of the striated portion, increase.

The author has observed a similar process in the growing extremities of the sheep and of man, and concludes that the growth of muscular fibre commences in the cells of the embryo by the elongation of the cells and multiplication of their nuclei, attended by a simultaneous fibrillation and striation of their contents. He regards the sarcolemma as resulting from the extension of the wall of the parent cell, but thinks that the adult muscular fibre should not be regarded so much in the light of a single many-nucleated cell, as the result of the fusion of many cells in the act of formation, the separation of which, after the division of their nuclei, has been prevented by the early fibrillation of their contents,—a view which approximates somewhat to that held by Schwann, and which is also a modification of the opinion expressed by Kölliker and Remak.

The development of the muscular fibre of the heart in the chick commences, according to the author, after forty-eight hours of incubation, by the appearance of stellate cells, which anastomose with one another in all directions. The processes which these give off increase in thickness, and nuclei appear upon them, probably by multiplication of the nuclei of the original cells. Fibrillation and transverse striation of these processes appear from the third to the fourth day. The structure becomes so complex after this period, that the author has been unable to follow the development further. He has not been able to find any membrane resembling the sarcolemma upon these processes from the stellate cells, though with a power of 1250 linear they may often be seen to have a double outline. He thinks the presence of a sarcolemma may be inferred from the fact that the position of the nuclei in relation to the striated portion is the same as

in other striated muscle, and that its excessive tenuity is probably the cause of its escaping observation.

XVI. "Researches on the Structure, Physiology, and Development of *Antedon* (*Comatula*, Lamk.) *rosaceus*." By Dr. W. B. Carpenter, F.R.S. Received June 15, 1865.

(Abstract).

The author, after adverting to the special interest attaching to the study of this typical form, as the only one readily accessible for the elucidation of the life-history of the Crinoidea, states it to be his object to give as complete an account as his prolonged study of it enables him to offer, of its minute structure, living actions, and developmental history, taking up the last at the point to which it has been brought in the memoir of Prof. Wyville Thomson.

He prefaces his memoir with an historical summary of the progress of our knowledge of the distinctive peculiarities of this genus, and of its relation to the Crinoidea; and he shows that the first recognition of this relationship was most distinctly made by Llhuyd, at the beginning of the last century, though that recognition has been passed without notice by most subsequent writers, and is altogether ignored by MM. de Koninck and le Hon in their recent history.

The author then proceeds to describe the external characters of *Antedon rosaceus*; and shows, from its habits as observed in a vivarium, that although possessed of locomotive power, it makes so little use of this under ordinary circumstances, that its life in the adult condition, no less than in its earlier stage, is essentially that of a pedunculate Crinoid.

He then gives a minute description of the several pieces of the skeleton—the accounts of these previously given by J. S. Miller and Prof. Joh. Müller not being in sufficient detail to serve as standards of comparison to which the parts of fossil Crinoids may be referred. And he directs special attention to the curiously inflected rosette-like plate, previously unnoticed, which occupies the central space left within the annulus formed by the adhesion of the first radials. This plate is in special relation to the organ termed by Joh. Müller the "heart," but certainly having no proper claim to that designation, being a quinquepartite cavity in the central axis, from the walls of which there pass out not vessels but solid cords of sarcode, into the rays and arms, and also into the dorsal cirri. The inflexions of the rosette-like plate serve for the support and protection of the large cords passing into the rays, each of which has a double origin, and a connexion with the adjacent radiating cords that reminds the anatomist of the "circle of Willis."

The skeleton of the adult differs so widely in the forms and relations of its parts from that of the early Pentacrinoid larva described by Prof. Wyville Thomson, that the derivation of the former from the latter can only be understood by observation of all the intermediate stages. When

the calcareous skeleton of the calyx first shows itself, it consists only of five *oral* plates arranged conformably upon five *basal* plates, as thus:—

O O O O O
B B B B B

At a stage a little more advanced (which has been described by Prof. Allman, Trans. Roy. Soc. Ed. vol. xxiii. p. 241), the rudiments of the *first radials* are found interposed between the orals and basals, alternating in position with both, as in the margin; and between two of these first radials there appears a single small unsymmetrical plate, which afterwards proves to be the *anal*. The first radials undergo a rapid increase in size, and soon become surmounted by *second* and *third* radials, which project between the orals; whilst the orals and basals, undergoing no such increase, are relatively very much smaller; the *anal* plate is still found on the line of the first radials. But from this time the radials form the principal part of the calyx, which opens out widely in conformity with the increase of space required for the digestive apparatus, the intestinal canal being now developed around what was originally a simple stomach with one orifice. The highest joint of the stem also undergoes a remarkable increase in size, and begins to acquire the form of a basin with an inflected rim, constituting what is known in the adult as the *centro-dorsal* piece. When the calyx opens out, the five *oral* plates which originally formed a circlet around the mouth, retain that position, and detach themselves entirely from the divergent radials, nothing but the soft perisomatic membrane filling up the space between them. These *oral* plates never increase in size, and towards the end of the Pentacrinoid stage they begin to undergo absorption. I can still trace their basal portions in young specimens of the free *Antedon*; but as the creature advances towards maturity they are altogether lost sight of. When the intestinal canal has been sufficiently developed to open on the surface of the oral disk, the *anal* plate is lifted out of the position it originally occupied, and is at last found on the anal funnel, far removed from the radials. This, like the oral plates, begins to undergo absorption towards the end of the crinoidal stage, and completely disappears in the early part of the life of the free Antedon. The *radial* plates increase not only in size but also in thickness; and channels which are left on their internal surface by vacuities in the calcareous network, are converted into canals by a further inward growth of this, which completely covers them in. It is through these canals that the cords of sarcode pass to the arms. The *basal* plates, like the oral, remain stationary in point of size, and present no change in appearance or position until after they have been completely concealed externally by the centro-dorsal piece (the highest joint of

O O O O O
a a a a a
B B B B B

A^3 A^3 A^3 A^3 A^3
A^2 A^2 A^2 A^2 A^2
O O O O O
A^1 A^1anA^1 A^1 A^1
B $\overline{B}$ B B B

the stem), which rapidly augments, both in absolute and in proportional size, when the development of the dorsal cirri is taking place from its convex surface. By the end of the Pentacrinoid stage, this plate has extended itself so far over the base of the calyx as completely to conceal the basals; and as the free Antedon advances towards maturity, it gradually extends itself over the first radials, which then become adherent to it and to each other. The basals then undergo a most curious metamorphosis, consisting in absorption in one part and extension in another, by which they finally become converted into five peculiarly shaped pieces, the ultimate union of which forms the single rosette-like plate, which has been already stated to lie within the annulus formed by the first radials of the adult *Antedon*. Hence the calyx finally comes to be thus composed:—

$$\begin{array}{c} R^3 \quad R^3 \quad R^3 \quad R^3 \quad R^3 \\ R^2 \quad R^2 \quad R^2 \quad R^2 \quad R^2 \\ \underbrace{b\ R^1\ b\ R^1\ b\ R^1\ b\ R^1\ b\ R^1}_{\text{CENTRO-DORSAL.}} \end{array}$$

As the *orals* and the *anal* have entirely disappeared, no part of the primordial calyx of the Pentacrinoid larva is traceable in it, until we separate the adherent pieces which form its base, and search out the minute and delicate rosette-like plate which is formed by the metamorphosis of the *basals*.

The structure, physiology, and development of the digestive, circulatory, and respiratory apparatus, and of the nervous and muscular systems, will form the subject of a future memoir.

XVII. "On the Chameleon's Retina; a further contribution to the Minute Anatomy of the Retina of Amphibia and Reptiles." By J. W. Hulke, Esq. Communicated by William Bowman, Esq.

(Abstract.)

The Chameleon's retina is peculiar in having a fovea and pecten, and in the nervous conducting fibres crossing the connective-tissue fibres instead of running parallel to them. The fovea was discovered by Knox in 1823, and minutely described by H. Müller, who also discovered the singular arrangement of the two sets of fibres in 1862. It is a circular pit situated at the posterior pole of the eyeball. A dark brown dot, surrounded by a lighter areola, marks its centre. Here the bacillary layer, which contains cones only, is alone present. The cones of the fovea are long, slender cylinders placed vertically upon the choroid. From the centre of the fovea outwards, the cones become stouter, shorter, and more numerous towards the periphery of the retina,

where they are flask-shaped. The other layers reach their maximum development around the fovea at successively increasing distances from its centre. From the inner ends of the cones, fine fibres proceed obliquely from the outer to the inner surface of the retina in a radial direction from the centre of the fovea to the periphery of the retina. These fibres connect the cones with the cells of the outer granule-layer; they next form a thick plexus at the inner surface of this layer, which I term the cone-fibre plexus; then traverse the inner granule-layer, in which they connect themselves with round and roundly oval cells, and are continued through the medium of the ganglion-cell-like cells of this layer into the granular layer, where they join the processes directed outwards from the cells of the ganglionic layer. *Thus they constitute an anatomical path between the cones and optic nerve-fibres.*

These oblique nervous fibres are crossed by vertical fibres of modified connective tissue directed radially from the centre of the eyeball. Around the fovea the connective fibres traverse the cone-fibre plexus and the outer granule-layer in the form of stout vertical pillars corresponding to those which in the turtle I named the outer radial fibres; while in the thinner periphery of the retina, the vertical, connective-tissue fibres are finer, and traverse all the layers between the inner and outer limiting membranes.

The pecten lies excentrically at 1‴ from the centre of the fovea. Its minute structure agrees with that of the Gecko's.

The distribution of the optic nerve-fibres with respect to the fovea resembles that which obtains with reference to the yellow spot in the human eye.

XVIII. "Additional Varieties in Human Myology." By John Wood, F.R.C.S., Demonstrator of Anatomy in King's College, London. Communicated by Dr. Sharpey. Received June 9, 1865.

In the past winter session thirty-six subjects have been dissected in the Anatomical Rooms at King's College. In them the author has directed especial attention to the combinations of muscular aberrations in the same individual, with a view to obtain data for ascertaining any relation that may subsist between such abnormalities in different parts of the body.

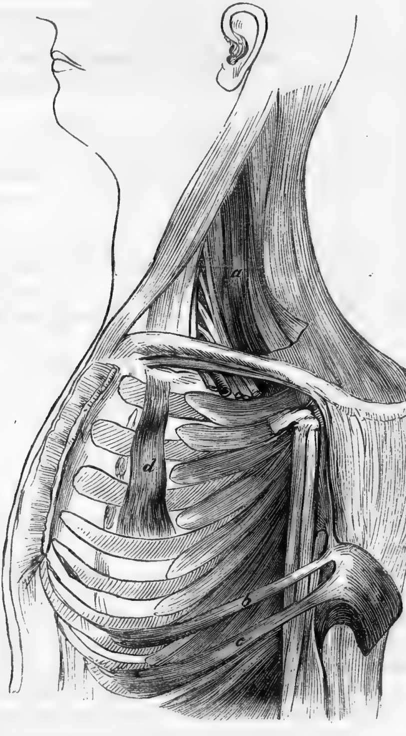
In one subject, a muscular man about 5 feet 8 inches high, with prominent features, aquiline nose, somewhat high cheek-bones, well-pronounced chin, and good skull-development, an extensive departure from the ordinary type was observed in every part of the body, the abnormalities being more numerous than in any other subject previously noted.

In the neck, on both sides, was a well-developed and powerful *levator claviculæ,* in all respects like that first described and figured by the author in a paper read last year before the Royal Society. It was connected

above with the posterior tubercles of the second and third cervical vertebral transverse processes, arising with the fibres of the *levator anguli scapulæ.* Passing downwards, forwards, and outwards, as a muscle about an inch wide, it was inserted into the outer third of the clavicle, behind the fibres of the trapezius muscle, and opposite the conoid tubercle of that bone.

The fasciculus was muscular in nearly its whole extent (fig. 1 *a*).

Fig. 1.



Arising from the hinder border of the first rib with the *sterno-thyroideus* muscle, and passing over the common carotid artery to be inserted into the cervical fascia at the upper part of the neck, was a *costo-fascialis cervicalis* muscle, precisely similar to that described and figured in the paper before alluded to.

In the axilla, on both sides, the *latissimus dorsi* sent a muscular slip three-fourths of an inch wide, in front of the vessels and nerves, to be inserted, with the upper sternal fibres of the *pectoralis major,* into the outer bicipital ridge of the humerus (fig. 1 *c*). A similar detached slip arose from the seventh rib, close below the *pectoralis major,* and was inserted separately into the bicipital ridge a little higher than the foregoing (fig. 1 *b*).

From the outer border of the first rib, near the cartilage, arose a thin, fleshy, triangular muscle which, widening gradually, dropped fibres of insertion into the second, third, and fourth ribs, close outside the origin of the *pectoralis minor.* It was entirely distinct from the intercostals, and may be termed a *supra-costal* muscle. It existed on both sides, but was more marked on the left (fig. 1 *d*).

In the upper arm was a well-marked *brachio-fascialis,* exactly similar to that described in the last paper, arising with the upper fibres of the *brachialis anticus,* and inserted into the semilunar fascia of the elbow, intervening between the brachial artery and median nerve close above the bend of the elbow.

In the right arm only was a large fusiform muscle, arising, by a thin lunated aponeurotic tendon, from the oblique line of the radius under the origin of the *flexor sublimis,* and inserted by a narrow spreading tendon into the deep surface of the anterior annular ligament close to the trapezium.

Some of the fibres could be traced into the middle portion of the palmar fascia.

This muscle was also found in another muscular male, associated, as in this case, with a remarkably developed *extensor brevis digitorum manus.* It seems to be a homologue of the *tensor fasciæ plantaris* given in the series of drawings accompanying the last paper.

A strong and distinct *palmaris longus* and *brevis* were also present. There was increased differentiation of the *flexor sublimis digitorum.*

The *flexor pollicis longus* gave a strong muscular slip to the indicial portion of the *flexor profundus digitorum.*

The third *lumbricalis* was double, half going to the third and half to the fourth digit, and implanted in the usual manner into their opposed sides. This was also seen in another subject.

There was an *extensor proprius digiti medii* from the lower end of the back of the ulna and interosseous ligament, distinct from the indicator muscle, and inserted into the dorsal expansion of the common *extensor* tendon, lying on its deep surface and sending lateral slips to the metacarpo-phalangeal ligaments.

The *extensor ossis metacarpi pollicis,* on both sides, had three distinct tendons, one to join the *abductor pollicis,* another to the front of the trapezium, and the third, the largest, to the base of the metacarpal bone. This is a common arrangement.

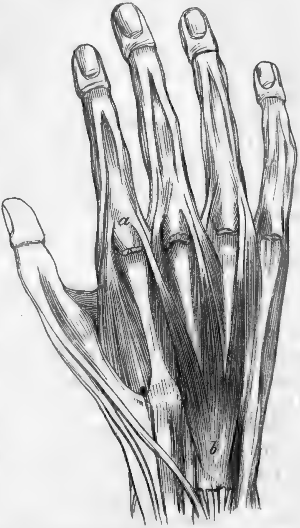
The left *abductor pollicis* was a double muscle, which is also commonly found.

On the back of both hands was a good specimen of the muscle first described and figured by the author, in his last paper, as an *extensor brevis digitorum manus.* It was arranged in three slips, arising by a common aponeurosis from the magnum and unciform bones, the two outer inserted with the *dorsal interossei* muscles into the extensor aponeurosis at the base of the middle finger; and the inner, into the same structure at the base of the ring-finger.

This muscle was also found very well marked in another muscular male arm, associated with the fusiform deep *palmaris* just described.

Fig. 2 is drawn from this specimen, and it will be seen that in it there is a still closer approach to the arrangement of the *extensor brevis digitorum pedis,* inasmuch as the outermost slip is not inserted with the second dorsal interosseus into the middle finger, but with the first palmar interosseus into the ulnar side of the index or second digit and its extensor aponeurosis. This specimen has been preserved as a preparation for the Hunterian Museum of the Royal College of Surgeons, where it may be inspected by those interested in the question.

Fig. 2.



In the foot of the subject first mentioned, the *tibialis anticus* on both sides, sent forwards a tendinous slip, one-eighth of an inch wide, to be inserted with the tendon of the *extensor proprius hallucis* into the base of the first phalanx of the great toe. (This was also found in a female subject on both sides.)

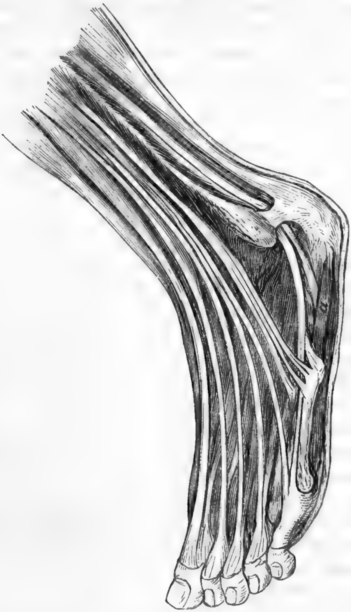
The *peroneus brevis* sent off a tendinous slip (*peroneus quinti*) to the extensor aponeurosis of the little toe on both sides.

The *peroneus tertius,* on both sides, had a very broad tendon, which was inserted into the base of the fourth as well as the fifth metatarsal bone.

The same peroneal disposition (*tertius* and *quinti*) was also observed in another muscular male foot, with an additional peculiarity which caused it to be selected as the subject of fig. 3, where it is seen at *a.* In both these subjects an *abductor ossis metatarsi minimi digiti* was present on both sides. In

the subject of the figure, it was the largest specimen the author has met with since he first discovered the muscle as a frequent abnormality in the human foot.

Fig. 3.



In the second metatarsal space, both the bones forming its sides gave origin to both the *plantar* and *dorsal interossei* muscles, producing the appearance as if the dorsal interosseus proper were divided between the second and third digits.

The arteries of the arm in this subject were generally irregular. There was an axillary origin of the radial, and the superficial arch supplied the index and pollex by the aid of a large superficial volar.

We have thus in this remarkable subject a development of a true *levator claviculæ,* such as is found in all kinds of apes, monkeys, and bats, and offsets from the *pectoralis major* and *latissimus dorsi* similar to the *chondro-* and *dorso-epitrochlear* found also in these animals and the moles and birds.

We have further a *brachio-fascialis* or quasi third head of the *biceps* usually found in birds; a muscular connexion between the *flexor pollicis*

longus and *flexor digitorum profundus,* as found in the apes and monkeys; with a curious addition of the nature of a *tensor fasciæ palmaris,* forming a close homologue with the *plantaris flexor* found in many of the lower animals; a double *lumbricalis,* as often seen in the apes; and a proper extensor of the middle finger. There is a redundancy of the *extensor ossis metacarpi pollicis* and *abductor pollicis,* and an *extensor brevis digitorum* on the back of the hand. This last curious muscle the author has now traced in all stages of its segregation and posterior displacement from the fibres of the dorsal *interossei,* which indicate strongly the light in which we should view this muscle on the dorsum of the foot. (In the fore paw of the Sloth, Professor Huxley has shown the author a similar displacement and use of the *dorsal interrossei* as extensors of the digits, while the *palmar,* as in most of the lower animals, fulfilled the part of *flexores breves* as well as divaricators of the digits. This function in the Sloths is rendered necessary by the imperfect development and abnormal displacement of the tendons of the *extensor longus.*) Lastly, in the foot of this subject we have the *tibialis anticus* and *peroneus brevis* muscles sending forwards tendinous slips to their respective digits (first and fifth). A special abductor of the metatarsal bone of the fifth digit, such as Professor Huxley and Mr. Flower have shown to exist uniformly in the higher and lower apes, and a double origin of the first plantar *interosseus* muscle, complete the list of irregularities which render the above subject one of the most remarkable the author has ever dissected.

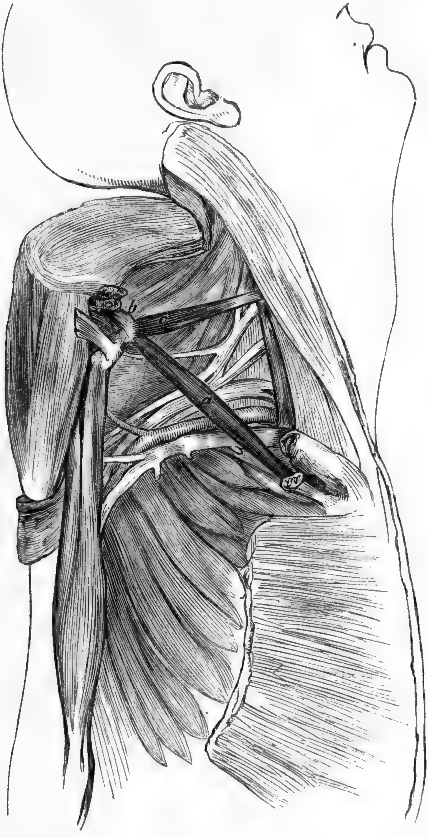
In a thin female subject of low stature was found, on the right side only, the remarkable muscle given in fig. 4. It consisted of a roundish fusiform slip (*a*) arising tendinous from the first cartilage below the *subclavius* close to the *manubrium sterni,* passing across the subclavian vessels and nerves quite distinct from the last-named muscle, and inserted into the upper border of the scapula and suprascapular ligament, where it was connected, to some extent, with the origin of the *omo-hyoideus* (*c*). From this point of insertion another slip of muscular fibres passed forwards, upwards, and outwards, to be inserted, with the *subclavius,* into the outer third of the clavicle (*b*).

This muscle seems to be the same as that given in the author's first series under the name of a double *subclavius,* with the addition of a connecting slip to the clavicle. It seems to the author to represent pretty closely the *sterno-scapular* muscle, while contributing to support the thorax in the pachyderms and ruminants, in which animals it is continued as far as the manubrium.

In the same subject was a slip, on the left side only, arising from the eighth rib, with the digitation of the *serratus magnus,* and inserted into the short head of the *biceps* close to the coracoid process. A rather larger muscle like this was described and figured in the first series, under the name of a *chondro-coracoid* muscle. There was a third head of the *biceps* on the left side, arising with the *brachialis anticus,* and on both sides

a scapular head of the *latissimus dorsi,* and a tendinous slip from this muscle to the long head of the *triceps.*

Fig. 4.



In the left arm of this subject, was found, for the fourth time, the curious muscle first described in the author's last paper as the *extensor carpi radialis accessorius,* arising by a broad fleshy head from the external condyloid ridge of the humerus, below and distinct from the *extensor carpi radialis*

longior, and inserted by a long tendon into the base of the metacarpal of the pollex, below and distinct from the *extensor ossis metacarpi pollicis*. In this instance no slip was given to the *abductor*, as is sometimes the case. The author had the satisfaction of showing this specimen to Professor Sharpey, with the *levator claviculæ* before described. Professors Ellis and Huxley, and Messrs. Flower and Pettigrew of the Royal College of Surgeons, also inspected it. It was not present on the right side, but here a muscular connexion existed between the *supinator longus* and *extensor carpi radialis longior*. There was no *palmaris longus* on the left side, and only a small one on the right. On the left side also the fourth *lumbricalis* was absent.

In the body of a fine young Negro, which was very carefully dissected and observed, few departures from the ordinary muscular arrangement were observed, and these were present only in the upper extremity. In the left arm was a complex arrangement of the *flexor sublimis digitorum*. Two large muscular slips from the coronoid origin of this muscle passed to the tendons of the deeper muscles. The inner and more superficial terminated in two long tendons, which passed separately under the anterior annular ligament, and became blended in the middle of the palm with those of the *flexor profundus* going to the fourth and fifth fingers. The outer slip also divided (a little higher up) into two tendons. One of these joined, in the middle of the forearm, that of the *flexor pollicis longus*; and the other, after receiving a muscular head from the radius below the last-named muscle, became connected in the palm with the perforating tendon of the index, giving part origin to the first lumbricalis. Here were three additional tendons intermediate between the *flexor sublimis* and *profundus*, passing separately under the annular ligament. Additional tendons have been before met with in this position in Europeans, but the author does not remember to have met with them to the extent seen in this Negro. In the same arm, the third lumbricalis joined the ulnar side of the middle finger instead of the radial side of the ring-finger, which had no lumbricalis. The interossei muscles were regular, that to the thumb (the first palmar of Henle) was also present. All the *palmares* muscles were well developed, as well as the *plantares* and the *peroneus tertius*. The latter was connected at its origin (as is commonly found) more intimately with the *extensor tendons* of the fourth and fifth toes, than these were with those of the second and third.

The *arteria comes nervi mediani* was very large, forming the greater part of the superficial palmar arch, and supplying the thumb and index.

In a well-formed tall adult Lascar, with a good cranial development, features of an elevated type, and of a deep olive colour, the most careful observation detected no further irregularity than an *extensor proprius* of the middle finger on both sides, arising partly in common with the *indicator*, and inserted into the common extensor aponeurosis. There was also an increased differentiation of the *flexor sublimis digitorum*.

In two muscular male subjects were found a well-marked *sternalis brutorum*, very similar to that figured in the last series, and in both (as in that case) on the right side only. In another male it was found on the left side only; and in a fourth, slips of tendon, intermingled with muscular fibre, were found on both sides, passing from the sternal tendon of the *sterno-mastoideus* down to the cartilages of the ribs as low as the sixth, and evidently of the nature of a *sternalis* muscle. Two of these subjects were affected with further abnormalities, confined to the arms. In the right arm of one was found the *tensor fasciæ palmaris* before described, and associated with the *extensor brevis digitorum manus* (given in fig. 2). The latter was present in both hands. The *palmaris longus* on the left side was much stronger than that on the right. In the right arm also was a muscular slip connecting the *flexor profundus* with the *flexor longus pollicis*, a double *indicator* muscle, and no less than three *extensor tendons* to the little finger. In the subject in which the *sternalis brutorum* existed on the left side only, were found, in both arms, slips connecting the *flexor sublimis* with the *flexor longus pollicis*, and a distinct muscle, arising from the radius inside the last muscle, and becoming connected, by means of a long and strong tendon, with the perforating or deep tendon of the index just below the annular ligament, precisely similar to one given in the last series. On the dorsum of both hands were found three well-marked and distinct muscular slips, forming an *extensor brevis digitorum*, arising in common as high as the posterior annular ligament. Small slips representing these, and passing to the middle and ring-fingers only, have been found in no less than six other subjects during last session.

In another male left arm were found combined the following abnormalities, viz. three heads to the *biceps*, a double *palmaris longus*, and a double tendon to the *extensor minimi digiti*. Right arm normal.

In two subjects were seen, in the legs, good samples of the *extensor primi internodii hallucis*, distinct muscles, with a strong tendon, as described and figured in the last series. In five subjects were found, on both legs, tendinous slips representing the *peroneus quinti*. In that from which fig. 3 was taken (a very tall and muscular man), it will be seen that the digital slip passes in a curious way through a division of a large tendon of the *peroneus tertius*, at its insertion into the bases of the fourth and fifth metatarsals. It is associated also with an *abductor ossis metatarsi minimi digiti*.

In connexion with these more common irregularities of the peroneal tendons, the author would call attention to that given in fig. 5 from a left female foot, in which the tendon of the *peroneus longus* (*a*), as it turns over the cuboid bone, gives distinct and sole origin to the *flexor brevis minimi digiti* (*b*), and to the outermost *plantar interosseus* of the same digit (*c*).

Of other detached muscular abnormalities observed during the session,

the more remarkable may now be briefly described.

Fig. 5.

In a female was found, on both sides, an increased development of a common irregularity, viz. a broad muscular slip from the tendon of the *latissimus dorsi,* passing across the axillary vessels and nerves to be inserted with the deeper or sternal fibres of the *pectoralis major.* This slip was separated from the rest of the *latissimus* by a well-marked tendinous intersection, and was connected with the ninth rib. In a male subject, which presented an abnormal subclavian slip of muscle closely resembling that in fig. 4, were found upon the larynx two small but curious muscular slips arising from the lower border of the thyroid cartilage on the left side, between the *crico-thyroid* and *thyro-hyoid* muscles, and passing obliquely across the median line, in front of the thyroid isthmus, to be inserted into the front of the fifth ring of the trachea, near to and parallel with each other. They seemed to be prolongations of the superficial fibres of the *crico-thyroideus,* with the tendency to cross the median line more commonly shown by the hyoid and laryngeal muscles than elsewhere.

In a male pharynx, the middle constrictor showed an irregularity. A few of the upper fibres, on both sides, arose from the vaginal process of the temporal bone, and, curving inwards and upwards, were inserted with the rest of the upper fibres of the constrictor into the pharyngeal ridge and median raphe. This arrangement is somewhat different from that of the *salpingo-pharyngeus* described by Cruveilhier and not unfrequently found in this situation.

In both arms of a muscular male was found a small slip of tendon giving off a fourth head to the *biceps,* and springing from the *lesser* tuberosity of the humerus at the insertion of the capsule and tendon of the subscapularis. This is a bicipital head of the same character as the fourth head described by Meckel as arising sometimes from the *greater* tuberosity at the edge of the bicipital groove. The third head in the present case arose in the usual situation, from the upper fibres of the *brachialis anticus.*

In a feebly developed male left arm was found a curious offset from the *flexor pollicis longus.* On its inner side, arising partly in common with this muscle, was a penniform muscle of good size, ending in a long and strong tendon which, after passing under the annular ligament, became continuous with the outer of the two heads of a double *first lumbricalis*

muscle. The other head was derived in the usual penniform way from the indicial tendon of the *perforans.* The whole muscle was larger than common, and was inserted in the usual way.

The same hand presented also a double insertion of the third *lumbricalis,* which was divided between the inner side of the medius and the radial side of the ring-finger, and inserted in the usual way. The middle finger is thus provided with a *lumbricalis* on each side. An exactly similar arrangement to this was found in another subject, a female, on both sides.

In a muscular male, the *extensores radiales* of the left arm exchanged tendinous slips of considerable size. That from the *longior* was highest, and joined the *brevior* just below the place where the latter gave off its return slip to join the tendon of the *longior* at its insertion into the base of the second metacarpal. Mr. Langmore, a student of University College, has lately written to the author to say that he has seen in a subject there dissected, a muscle arising with the *extensor carpi radialis longior,* the tendon of which, passing under that of the *brevior,* was inserted to its ulnar side into the base of the middle metacarpal. These irregularities are interesting in their bearing upon the occasional occurrence of the *extensor carpi radialis accessorius* before described. This muscle, however, is distinguished from all these by its insertion into the metacarpal of the *pollex,* and its frequent connexion with the *abductor* in the manner of the tendon of the *extensor ossis metacarpi.*

In many feet of both sexes, examined during the session, were found sesamoid bones developed in the tendon of the *tibialis anticus,* and playing over a bursa on the internal cuneiform cone. In one, a male, a strong distinct slip of tendon passed from it to join and strengthen the inner division of the plantar fascia, being ultimately attached to the base of the great toe.

In many of the same feet, and in others, a sesamoid bone was likewise found in the tendon of the *tibialis posticus,* placed to the inner side of, and playing over, the scaphoid bone. Its relation to the occurrence of an additional tarsal bone in this situation in the hinder limbs of the Armadilloes and other Edentata is suggestive. The special muscle found attached to it in these animals is produced apparently by a differentiation of fibres of the *tibialis posticus,* similar to that which frequently occurs in the *tibialis anticus* in the human subject, as shown in the author's last paper read before the Society.

In a small male foot (right) was found a slip of muscle revealing a transitional formation towards that universal in the apes, and sometimes seen complete in the human subject. A small slip of muscle from the *flexor brevis digitorum* (fig. 6 *b*) is joined by a similar one, arising by a tendinous origin from the outer part of the tendon of the *flexor longus* (*a*). The two, after joining, result in a tendon, which instead of forming a regular *perforatus* for the little toe, becomes blended with that of the long or *perforating flexor* at the first phalanx, giving off slips

only to the middle and ungual phalanges. On the other foot no abnormal muscle, but a similar blending of the tendons of the little toe was found.

Attention having been drawn by Mr. Huxley to the importance of ascertaining the arrangement of the *interossei* muscles in the hand and foot, and especially the usual or most frequent manner of insertion in the toes in the human subject, the author has carefully examined these muscles in a considerable number of subjects. It was found that in the hand, although the origin of these muscles is usually such as described in anatomical works, viz. of the dorsal by a double penniform arrangement from the adjacent metacarpals, and of the palmar by a single penniform origin from the metacarpal of its own digit, yet in several cases the so-called first *palmar interosseus*, viz. that of the index, had a bipenniform origin from both second and third metacarpals, exactly as that on the corresponding side of the same digit in the foot. This abnormality is sketched in fig. *7 a*. The *dorsal interosseus* of the same space covers it by its double penniform origin (one portion of which is represented divided in the sketch).

Fig. 6.

Fig. 7.

Both the muscles are perforated by the arterial branch of communication from the dorsum to the palm. In this hand is also well seen the *palmar*

interosseus of the thumb (*b*) exposed by the division of the *abductor indicis,* and lying upon the *flexor brevis,* with the deep fibres of which it is usually confounded.

The insertions of these muscles are invariably (as usually described, and as seen in the sketch) divided between the base of the phalanx (where it is blended with the capsular investment of the joint derived from the extensor aponeurosis) and the sides of the extensor tendon, passing with the fibres from the *lumbricalis,* partly to the middle, and chiefly to the ungual phalanx.

In the foot, the same occasional reference to the type occurring in the hand is found, in the origin of the first *plantar interosseus.* This muscle is sometimes a double penniform, arising from the adjacent second and third metatarsals on the plantar aspect of the second dorsal, and, like it, perforated by the communicating artery. In both the hand and foot where these irregularities are found, the respective digits to which the muscles are attached seem somewhat larger in proportion than is usual, the size and extent of attachment of the muscles appearing to be determined by the size and uses of the corresponding digit. The foregoing abnormalities of the interossei reflect some light upon the differences in the normal arrangement in the upper and lower extremities, which have often perplexed anatomists. The terms *dorsal* and *plantar* or *palmar,* referring to position only, and not to the action of these muscles, have apparently somewhat obscured the homologies of the separate muscles.

In the hand, the middle digit being the most bulky, has a double or dorsal interosseous muscle for each of its divaricators. Its divaricator to the pollex excludes from the third metacarpal the divaricator from the pollex of the second digit, and obtains origin for itself from the dorsal part of the second metacarpal, so becoming a dorsal muscle. The transverse convexity of the back of the hand gives a dorsal prominence to the middle metacarpal and its digit over the rest. This explains the circumstance of this muscle assuming a dorsal position over the palmar interosseous of the index.

In the foot, the first and second metatarsals and their digits attain a greater proportionate size and dorsal prominence, to fulfil their chief function of sustaining and propelling the body. Here we find the divaricator to the pollex of the second digit (the first palmar interosseous of the hand) becoming developed into a double penniform muscle, with a dorsal position, excluding the divaricator to the pollex of the third digit (the second dorsal of the hand) from attachment to the second metatarsal, and itself acquiring an origin from the third metatarsal.

An occasional recurrence of one to the type of the other might have been expected under peculiar conditions of development. Mr. Huxley informs the author that he has found, almost invariably, that the interosseous muscles in the foot are inserted entirely into the bases of the phalanges, and are not, as in the hand, prolonged by a tendinous expansion in

common with the *lumbricales*, into the extensor aponeurosis, and so to the middle and extreme phalanges. He looks upon this as one characteristic distinction between the hand and foot. In the arrangement which the author believes to be almost general in respect to the insertion of the *interossei* in the foot, and which supports essentially Mr. Huxley's view, it will be found that the bulk of each tendon is implanted into the base of the first phalanx, blending with the lateral ligaments of the metatarso-phalangeal joint, while only a few of the dorsal fibres are sent upwards and forwards, to meet and blend with the slips sent down to the sides of the joint from the extensor aponeurosis. These are not, however, so distinct and powerful as we find them in the hand, and, in their thin and scattered appearance, differ entirely from the insertion of the *lumbricales* tendons into the more forward part of the same extensor aponeurosis.

"On New Cornish Minerals of the Brochantite Group." By Professor N. Story Maskelyne, M.A., Keeper of the Mineral Department, British Museum. Communicated by A. M. Story Maskelyne, M.A. Received February 13, 1865*.

In March last my attention was drawn to a very small specimen of Killas, with some minute blue crystals on it, associated with a few equally small green crystals. The latter I proceeded to investigate with the goniometer. They proved to have almost identical angles with Atacamite, and, presuming them to be crystals of that mineral, I neglected them in order to measure the angles of the blue crystals. These proved also to belong to the prismatic system, and evidently were a new mineral. The specimen had come to the Museum from Mr. Talling, of Lostwithiel, a dealer from whom the National Collection has received a very large proportion of its finest Cornish minerals, and whose attention had been called to this specimen by the novelty of its appearance. Mr. Talling no sooner was apprised of the interest attached to his little fragment of Killas, than he set energetically about tracing it to its locality.

After a short time he succeeded in finding this locality; and though he has not yet divulged it, he soon forwarded other specimens to me at the British Museum. He has since found fine masses of the minerals, which are described in this memoir, and they are now in the collection under my charge.

The Killas which usually carries these minerals is of a very friable texture, often occurring as a breccia cemented by the minerals themselves, and at other times coated by them as incrustations.

Sometimes, however, they are found on it as minute crystals scattered over, and in direct contact with, the rock, or in a succession of layers deposited on it. In the latter mode of occurrence, the stone, whether Killas

* Read February 23, 1865: see Abstract, p. 86.

or, as occasionally, a quartzose vein-stone, usually presents on its surface a very thin glaze of a greyish-white colour, and endowed with a remarkable metallic lustre. On this a thin layer, sometimes but $\frac{1}{8}$th of an inch in thickness, of the blue crystals is met with, and on that a thicker agglutinated mass of the same mineral of rather a paler blue colour. Sometimes this paler variety exhibits a very fine sky-blue colour, and assumes the form of foliations with the appearance of small and extremely thin crystals, which are, in fact, an aggregate of crystals generally twinned, and in the form of laminæ.

Above the whole is occasionally seen a coating, varying in thickness from an eighth to half an inch, of a faintly bluish- or greenish-white mineral with a fibrous, and sometimes a slightly foliated, structure.

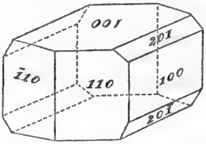
The place of the blue mineral is often taken by one of a fine green colour, varying from a dark emerald to verdigris-green, and often crystalline. Occasionally also crystals of Brochantite may be seen, sometimes in clusters, and occasionally also mixed with this green mineral.

I. *On Langite.*

The first of these minerals that I propose to describe is that which occurs in crystals and crystalline masses, whether of the deeper or lighter lines of blue. I propose to call it Langite, in honour of my valued friend and late colleague Dr. Viktor von Lang, now Professor of Physics at Gratz.

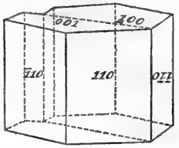
Langite crystallizes in very minute generally dark and somewhat greenish blue crystals belonging to the prismatic system, the ratios of the parameters being $a : b : c = 1 : 0{\cdot}5347 : 0{\cdot}6346$. The forms observed are (1 0 0), (0 0 1), (1 1 0), (2 0 1), and (0 1 0). The inclinations found between normals to thin planes being

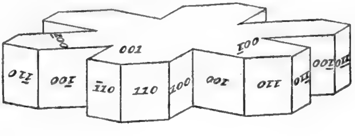
	°	′	
1 0 0 0 0 1 =	90		
1 1 0 $\bar{1}$ 1 0 =	56	16	
0 0 1 2 0 1 =	51	46	
1 1 0 2 0 1 =	68	8	
1 0 0 1 1 0 =	61	46	61° 52′ calc.
1 0 0 0 1 0 =	90		



The crystals are for the most part twinned similarly to those not unfrequent in cerussite: twin axes (1 1 0).

	°	′
$\bar{1}$ 1 0 (1 1 0) $\bar{1}$ 1 0 =	112	32
1 0 0 (1 1 0) 1 0 0 =	123	44
$\bar{1}$ 1 0 (1 1 0) 1 $\bar{1}$ 0 =	67	28





Cleavage parallel to 0 0 1 distinct; parallel to 1 0 0 nearly equally so. The plane 0 0 1 is brilliant; 1 0 0 rather less so, as is the rarer plane 0 1 0; the plane 1 1 0 sometimes exhibits hollows, the sides of which are parallel to the cleavages. The specific gravity of the mineral is 3·48 to 3·50. Its hardness less than 3. On looking through a section of one of these microscopic crystals of Langite, ground parallel to the plane [0 0 1] in the polarizing microscope, the plane of the optic axes is seen to be parallel to 1 0 0; but though coloured rings are visible, the axes lie beyond the field, and the double refraction is weak. Probably, however, the first mean line is the normal to 0 0 1, and it is negative. The symbol for its optical orientation would be

$$\mathfrak{b}\ \mathfrak{c}\ \bar{\mathfrak{a}}.$$

The crystals present dichroism :—

1st. As seen through 0 0 1 (along axis *b c*):

$\mathfrak{c}$ (plane of polarization ‖ to 1 0 0) greenish blue.

$\mathfrak{b}$ (plane of polarization ‖ to 0 1 0) blue.

2nd. As seen through 1 0 0 (along axis *a*):

$\mathfrak{c}$ (plane of polarization ‖ to 0 0 1) darker bluish green.

$\mathfrak{a}$ (plane of polarization ‖ to 0 1 0) lighter bluish green.

It is a fact worthy of remark that Langite is geometrically isomorphous with Leadhillite.

Langite is insoluble in water, but readily soluble in acids and ammonia. When submitted to the action of heat, it loses its blue colour, turning at first bright green. As the heat is increased, it passes gradually through various darker hues of this colour, till it becomes of a dull olive-green, and ultimately black. Water is given off the whole of the time, which in the later stages of the change has an acid reaction. Before the blowpipe, it gives off water and acid fumes, colours the flame green, and becomes reduced to metallic copper with carbonate of soda on charcoal. The chemical composition of Langite is represented by the empirical formula,

$$4Cu''O\ 5H'_2O\ SO_4.$$

which may be written as

$$3\left\{\begin{matrix} Cu''\ O \\ H'_2\ O \end{matrix}\right. + Cu''SO_4 + 2H'_2O$$

The copper was determined in one case (i.) by precipitation on the interior of a platinum crucible, by means of a cell of a Grove's battery, a method that seems, however, to give the value of the metal in excess; in other cases (ii., iii., and iv.) by means of the volumetric method, wherein the iodine set free on precipitation of copper by an iodide is determined by means of starch and hyposulphite of soda. The sulphuric acid was determined in the usual way. The water in two cases (i. and ii.), by mixing the powdered and dried mineral with previously ignited carbonate of barium and heating the mixture in a combustion-tube in a current of dry

air, the liberated water being retained by sulphuric acid in pumice in a bulbed U-tube. In the other cases the mineral was heated with oxide of lead, and the water estimated by the loss.

From the slight differences in the numbers given by the sulphuric-acid determinations in these minerals, and from the difficulty of determining the values of traces of iron, lime, and impurities as disturbing elements in the calculation of the analysis, the water-determinations are especially important. The following are the numbers the analyses have yielded me—1st, crystals of Langite, carefully selected; and 2nd, of the much paler blue incrustation or aggregation of crystalline Langite, generally showing the plane 0 0 1 largely developed on the surfaces of the incrustation:—

Copper-Determination.

I. Picked crystals.				
Grammes.				Per cent.
i. 0·2024	gave	0·1074	copper, corresponding to	53·06
ii. 0·2185	took	18·18	cub. centims. of standard solution of hyposulphite of soda, corresponding to*	52·26
iii. 0·1866	„	15·2	„ „ „ „	52·10
II. Pale Blue Langite.				
iii. 0·4295	„	35·8	„ „ „ „	52·80
			Average=	52·55

Sulphuric Acid-Determination.

I. Picked crystals.				
0·2746	gave	0·1297	Ba SO_4, corresponding to ·04457 SO_3	=16·20
1·6687	„	0·80475	Ba SO_4, corresponding to ·27587 SO_3	=16·23
0·288	„	0·1375	Ba SO_4, corresponding to ·0470085 SO_3	=16·35
II. Pale Blue Langite.				
0·1701	„	0·0826	Ba SO_4, corresponding to ·028387 SO_3	=16·61
0·4295	„	0·2094	Ba SO_4, corresponding to ·07196 SO_3	=16·75
			Average=	16·42

Water-Determination.

I. Picked crystals.				
i. 0·3622	lost	0·0649	Water, corresponding to	17·93
[ii. 0·4737	„	0·0915	„ „	19·31]†
iii. 0·7210	with oxide of lead	0·1326	„ „	18·39
II. Pale Blue Langite				
iv. 0·2258		0·0423	„ „	18·73
			Omitting the 2nd, the average=	18·317

There are traces of lime and iron; of the latter I found in one experiment 0·03 per cent.

* Two preliminary experiments with the standard solution on pure crystallized sulphate of copper and one on pure copper gave

$$\left.\begin{array}{l}99{\cdot}66 \text{ per cent.}\\ 99{\cdot}965 \quad „\\ 99{\cdot}92 \quad „\end{array}\right\}\text{of the copper required by calculation.}$$

† A little acid came over in this experiment.

The formula $3\,Cu''\,H'_2\,O_2 + Cu''\,SO_4 + 2H'_2\,O$ requires the following numbers * :—

			Calculated percentage.		Average found.
4 equivalents of copper	....	126·72	= 52·00		52·55
4 „	oxygen	32	= 13·13	(calc.	13·268)
1 „	sulphuric anhydride	40	= 16·41		16·42
5 „	water	45	= 18·46		18·317
		243·72	100·00		100·55

In order to determine the proportions of water on which the blue colour of the Langite depended, and, if possible, to obtain some insight into the nature, or, at least, the number, of the different degrees of the hydration, 1·6987 gramme of the crystals, after having been previously powdered and dried in dry blotting-paper, were heated in an air bath. The result was a loss :—

At	100° C. of ·02625 = 1·54	per cent. water		
Between	100° and 120 C. of ·03825 = 2·25	„	„	
	120 and 140 C. of ·03900 = 2·30		(begins to turn green).	
	140 and 180 C. of ·0620 = 3·650			
	180 and 190 C. of ·0692 = 4·216	„	„	
	190 and 220 C. of ·096 = 5·651	„	(turns dark olive).	
	250 C. of ·1352 = 7·959	„	„	
	255 C. of ·1402 = 8·254	„	„	
	260 C. of ·1472 = 8·616	„	„	
	290 C.	decomposition.		
	2 equivs. of HO = 7·384.			

The passage, then, of Langite, under the influence of heat, into a substance with the formula $3\,Cu''\,H'_2O_2 + Cu''\,SO_4 + H'_2\,O$ would take place at a temperature of about 180° C.; and it would further pass into a substance with the formula of Brochantite, $3\,Cu''\,H'_2\,O_2 + Cu''\,SO_4$ at a temperature of about 230° or 240° C.

A transition, however, so effected would probably be incompatible with a new crystalline structure in the mineral resulting from it, which would be, in fact, a pseudomorph.

The high temperature requisite for the expulsion of the last three equivalents of water, which cannot be performed without decomposition, would seem to give colour to the belief that this water is in more intimate association with the oxide, and forms with it a hydrate.

It is a fact worthy of remark that I have found one old specimen in which Langite is associated with Connellite. I convinced myself that the mineral was Langite by removing a crystal and measuring it. It gave the

* I have adopted in this paper the doubled equivalents of all the elements involved in my formulæ, except hydrogen.

angle of the prism 1 1 0, $\bar{1}$ 1 0=56° 34′, $\bar{1}$ 0 0 (1 1 0) $\bar{1}$ 0 0=124° 10′ (calculation requires 123° 44′).

II. *Waringtonite.*

The mineral to which I would next invite attention is one with a colour varying from emerald to verdigris-green that occurs sometimes mixed with Langite, but more often forming with it a continuous coating on the Killas or vein-stone, one part of this coating being in such cases Langite, and another part of it consisting of the mineral in question. At first I was in doubt whether this green body was not the result of the action of heat on Langite—in fact a pseudomorph after that mineral.

Subsequently, however, Mr. Talling sent me some unmistakeably crystalline specimens, and as at that time I had already made its analysis, there could no longer be any doubt that it was a new mineral.

I propose to call it "Waringtonite" in honour of my friend Mr. Warington Smyth, Inspector of Mines to the Crown Lands, and to the Duchy of Cornwall, &c.

The crystallography of Waringtonite presents a difficult problem, for the reason that it carries only one very distinct plane. The prevalent form of the crystals, which are almost microscopic, is that of a double curved wedge (or, to use a familiar illustration, like a stonemason's double-edged hammer), the flat summit being formed by this distinct but narrow plane. That plane is characterized by great brilliancy, is bounded by curved outlines, and though often fissured near its extremities by the accumulation of two or more parallel crystals in optical contact at their centres, is otherwise without striation. If we call this plane, by its analogy to the brilliant and unstriated plain in Langite, 0 0 1, and refer a very minute plane occasionally seen on the acute edges of the wedge to the form 1 0 0, we find the planes 0 1 0, 0 $\bar{1}$ 0, and those in the zone [0 1 0, 0 0 1] represented by rounded surfaces, from which it is impossible to obtain any accurate measurements; and the prism planes in the zone [1 0 0, 0 1 0] are likewise much curved. There would seem to be two prisms in that zone, one of which forms a normal angle approximately determined as 1 1 0, $\bar{1}$ 1 0=28° 30′ very nearly.

It is difficult to say whether Waringtonite is prismatic or oblique. The plane 0 0 1 forms an angle of 90° with those in the zone [0 1 0, 1 0 0]; and the principal planes indicated by the planes of polarization, as seen on looking down the normal to 0 1 0, are parallel to 1 0 0 and 0 0 1; but it is very difficult to speak with certainty as to the exact directions of the planes of polarization as seen when looking through the plane 0 0 1, and as to the direction of a plane of polarization really bisecting the acute angles of the wedge.

The crystals often occur in interpenetrating forms, with the appearance of being twins.

The angles, however, between corresponding planes in the two individuals are not sufficiently uniform for the establishment of a twin plane.

The analysis of Waringtonite yields numbers that conduct us to the formula $3\ Cu''\ H_2'\ O_2 + Cu''\ SO_4 + H'_2\ O$, as the results which follow suffice to prove.

Copper-Determination.

	grammes.		per cent.
i.	·1925	yielded (by precipitation) . . . . ·1054 copper	=54·75
ii.	·334	took 29·20 c.c. of standard solution of hyposulphite sodium .	=54·44
iii.	·320	„ 27·40 „ „ „ „ .	=54·252
		Average	=54·48

Sulphuric Acid-Determination.

i.	·2104	yielded ·1001 grm. $Ba SO_4$	=16·22 SO_3
ii.	·4201	„ ·2060 „	=16·825
iii.	·3200	„ ·16017 „	=17·16
		Average	=16·73 SO_3

Water-Determination

(3)	·4041	amorphous substance yielded when ignited with carbonate of barium . . . . ·0607	=14·18
2	·4891	picked crystalline Waringtonite . . . . ·0777	=15·00
1	·3897	do. do. . . . . ·0503	=14·80
4	·5680	lost when ignited with oxide of lead . . . . ·0806	=14·19
5	·3707	do. do. do. . . . . ·0540	=14·56
		Average, omitting the first,	=14·64

I am indebted to Mr. Madan of Queen's College, Oxford, for the analyses i. and ii. in this Table, and the water-determination in analysis i. of Langite.

Like Langite, this mineral also contains traces of iron, lime, and magnesia. Of protoxide of iron I found in a very pure crystalline specimen of the mineral 0·14 per cent.

Crystals of Brochantite are often mixed with the Waringtonite, and the more amorphous forms of the green substance would seem to be mixtures of the two minerals.

The formula $3\ Cu''\ H_2'\ O_2 + Cu''\ SO_4 + H_2\ O$ requires—

			Calculated percentage.		Average found.
4 equivalents of	copper	126·72	= 53·99		54·48
4 „	oxygen	32	= 13·63	(calc.	13·756)
1 „	sulphuric anhydride	40	= 17·04		16·73
4 „	water	36	= 15·34		14·64
		234·72	100·00		99·606

Like Langite, Waringtonite, though insoluble in water, is readily dissolved by acids and ammonia, and its behaviour before the blowpipe is similar to that of Langite.

Its specific gravity is 3·39 to 3·47.

Its hardness is 3 to 3·5. It abrades calcite, but not Arragonite. When a crystalline fragment is crushed between a cleavage face of celestine and a

smooth surface of porcelain or chalcedony, it leaves the celestine without perceptible abrasion. Brochantite, on the other hand, deeply cuts into that mineral.

In comparing the physical characters of these two minerals, one has furthermore to observe that, besides their differences in hardness and specific gravity (in Brochantite G=3·87–3·9), their crystallographic habits are entirely dissimilar. Thus if we assume, for comparison's sake, the angle obtained for the normal inclination of the planes 1 1 0, $\bar{1}$ 1 0 in Waringtonite to correspond to that between *e e′* or 1 0 1, $\bar{1}$ 0 1 of Brooke and Miller in Brochantite, a point of view from which we see the two minerals in the most advantageous orientation for comparison, we shall find that the planes of the form (1 0 1) in Brochantite, like those of (1 1 0) in Waringtonite, are much curved; the plane 1 0 0, however, is a well-marked plane in Brochantite, striated parallel to the zone-axis [0 0 1]. In Waringtonite the corresponding plane, 0 1 0, is a curved surface without striation. The plane 0 0 1 is furthermore a most conspicuous plane in the latter mineral, while the analogous plane 0 1 0 in Brochantite is, I believe, unknown.

A mineral, described by Berthier (Ann. Chim. Phys. l. 360), and one recently analyzed by Domeyko (Annales des Mines, 1864, p. 460), gave the following percentage composition:—

	Berthier.	Domeyko.	M. Pisani.	Waringtonite.
Copper	52·85	55·89	54·9	54·48
Oxygen	13·35	14·15	13·9	13·756
Sulphuric anhydride	16·6	16·15	17·2	16·73
Water	17·2	13·81	13·2	14·64
	100·0	100·00	1·0	99·606
			CaO ·8	
			101·0	

In the third column of the above Table I have also given the results of M. Pisani's analysis of a green mineral which he found associated with Langite, and which was probably Waringtonite mixed with the ferruginous Killas (on which it often occurs). He assigned the mineral to Brochantite. Berthier's mineral from Mexico was probably Waringtonite containing hygrometric moisture, as by deducting two per cent. of water his analysis almost exactly accords with the numbers representing that mineral. The green fibrous mineral from the Cobre mines in the Atacama desert would seem, from the description of the eminent mineralogist of Chili, to be a mixed substance.

III. *Atacamite.*

I have mentioned that the first specimen of Langite that came into my hands had upon it small bright green crystals of a mineral with the angles of Atacamite. These angles were the following:—

	Found.	Corresponding angles in Atacamite.
1 1 1, 1 0 0	=63° 48′	63° 20′
1 1 1, 1 1 0	=36 27	36 18
1 0 0, 1 $\bar{1}$ 0	=56 35	56 10
1 1 0, $\bar{1}$ 1 0	=66 50	67 40
1 0 1, 1 0 0	=52 50	52 50
1 0 1, 1 0 1	=74 20	74 20

Seen in polarized light through 1 0 0, the normal to 1 0 0 appears to be a bisectrix, and the plane of the optic axes is parallel to the edge 1 1 0, 1 0 0; and the crystal, as seen through 1 0 0, is negative.

It is dichroic, exhibiting—

𝔠, =plane of polarization parallel to 0 0 1, grass-green.

𝔟, =plane of polarization parallel to 0 1 0, more yellowish green.

There were but a very few of these minute, in fact microscopic crystals; but two of them I dissolved in nitric acid on a watch-glass, and tested them with nitrate of silver in the field of the microscope. A white cloud was at once struck in the solution, which, while refusing to dissolve in nitric acid, readily yielded to the solvent action of ammonia. This mineral then is Atacamite, as is confirmed by its apple-green streak. Since that time a mine in St. Just has produced this mineral, and I have from Mr. Talling a specimen from there which contains sulphate as well as chloride of copper. I hope soon to have the opportunity of effecting its analysis from purer specimens than such as have as yet been raised; for these consist of an intimate mixture, in which Atacamite, indeed, seems to be the preponderating ingredient, but in which, perhaps, Langite and Brochantite will prove also to be present.

"On the Rate of Passage of Crystalloids into and out of the Vascular and Non-vascular Textures of the Body." By Henry Bence Jones, A.M., M.D., F.R.S. Received April 26, 1865*.

It occurred to me that possibly, by means of the spectrum, I might trace the rate of passage of medicines into the vascular and non-vascular textures, and prove their presence, and determine the time during which they remain in action in some of the tissues far more accurately than had yet been done.

I was fortunate enough to obtain the assistance of Dr. A. Dupré, who had already published a paper in the Philosophical Magazine on the presence of lithium and strontium in the waters of London; and I am greatly indebted to him for carrying out all the suggestions which I thought requisite for proving how soon the salts of lithia pass into the different vascular and non-vascular textures of animals and of man, and how quickly

* Read May 4, 1865; see Abstract, p. 220.

these salts again pass out and cease to be detectable in the different parts of the body.

I shall divide this paper into five sections :—

1. On the method of analysis, and its delicacy.
2. Experiments on animals to which salts of lithium were given, upon the rate of their passage into the textures.
3. On the rate of the passage of lithia-salts out of the textures.
4. On experiments on healthy persons, and on cases of cataract.
5. On the presence of lithium in liquid and solid food.

1. *On the Method of Analysis, and its delicacy.*

Three methods of preparing the substance to be analyzed were followed, according as much or little lithia was present.

When plenty of lithia was present, it was immediately detected in the spectrum by simply touching the substance containing lithia with a red-hot platinum wire. In the case of liquids, a portion of a drop was taken up on the end of the wire, and it was then put into the gas-flame.

If no lithia was thus detectable, a larger or smaller portion of the substance was extracted by distilled water twice or thrice, and the liquid was evaporated to dryness, and the residue was then tested.

If very little lithia was present, it was necessary to incinerate a larger or smaller portion of the substance, and to treat the ash with sulphuric acid, to exhaust the resulting sulphates with absolute alcohol and evaporate the alcohol extract to dryness, and to test the residue for lithia.

Kirchhoff and Bunsen state that less than $\frac{9}{1,000,000}$ of a milligramme of carbonate of lithia = to about $\frac{1}{8,000,000}$ of a grain can be detected by the spectrum analysis.

To determine the delicacy of the test for the chloride of lithium, the following experiment was made :—One grain of chloride of lithium was dissolved in one litre of water. Of this solution 100 cub. centims. were taken and again diluted to one litre, this latter solution containing 0·1 grain of chloride of lithium to the litre.

When further diluted to five times its bulk, the lithium reaction was still seen faintly on a wire taking up 0·06 grain of solution. The line is most distinctly visible in the evening, in a somewhat dark room.

This dilution is equal to 0·1 grain of chloride of lithium in 5 litres of water, or 1 grain in 50 litres. 1 litre = 15,440 grains, or 50 litres = 772,000 grains. In 0·06 grain of this solution there are therefore 0·00000008 grain chloride of lithium, or about $\frac{1}{12,000,000}$th of a grain of chloride of lithium. This contains only $\frac{1}{6}$ part of lithium, so that the $\frac{1}{72,000,000}$th of a grain of metallic lithium, when pure, gives the spectrum reaction.

When the chloride of lithium was dissolved in urine, the test was from twice to six times less delicate than in distilled water.

2. Experiments on animals to which salts of lithium were given, upon the rate of the passage into the textures.

Experiment 1. Two guinea-pigs were fed for several days on the same food. One was killed, and the urine, the nails, hair, blood, bones, muscles, nerves, cornea, and crystalline lens were examined by the spectrum, and no trace of lithium was found anywhere. The other was given half a grain of chloride of lithium for seven days, and for two days one grain. It was then killed, and the lithium was found everywhere, even in the cornea, crystalline lens, hair, and toe-nails. In these it was more distinctly present than anywhere else, so that it probably came from the urine.

Experiment 2. Another guinea-pig, fed on the same food as the first two, was given only half a grain of chloride of lithium for three days. The third morning the lithium was detectable, by analysis, in the hair; the fourth day it was killed, and the lithium was found everywhere, as in the last instance.

Experiment 3. Another, after the hair and nails had been examined for four days and no lithium found, was given three grains of chloride of lithium. In two hours and a half lithium was detected in the hair of the belly, though in six hours none was found in the hair of the back; much more was then in the hair of the belly. In twenty-six hours it was killed. Lithium was found everywhere,—both in the outer and inner part of the lens very distinctly, and in the cartilage of hip- and knee-joints. The spleen and liver seemed to have less lithium than the vitreous and aqueous humour and the lens.

Experiment 4. A guinea-pig was given three grains of chloride of lithium, and in twenty-four hours it was killed. Lithium was found in the cartilage of the hip- and knee-joints, in the centre of the lens, in the nails, and in the outer moisture of the eye.

Experiment 5. To another, the hair of which gave no trace of lithium, were given three grains of chloride of lithium, and it was killed in eight hours; as usual, lithium was found in all the organs—by far the most in the kidneys. Little was found in the blood. It was quite evident in the cartilage of the hip-joint, and very distinct in the outer layer of the crystalline lens, but none at all could be found in the centre of the lens. Both lenses were examined more than six different times with the same result.

Experiment 6. In a guinea-pig, much younger than the last, which was killed eight hours after three grains of chloride of lithium, the whole lens was penetrated,—the smallest particle, even one-twentieth the size of a pin's-head, taken from each part of the lens, showing the lithium distinctly. The whole lens of another pig that had taken no lithium was burnt, and did not show the slightest trace of lithium.

Experiment 7. Another guinea-pig was given three grains of chloride of lithium, and it was killed in four hours. Lithium was found in the fibrin, serum, and corpuscles of the blood, in the cartilage of the hip-joint, and in

the lens, even in its most central part. There was scarcely any difference between the inner and outer part. The vitreous and aqueous humours showed much more evidence of lithium than the lens itself did.

Experiment 8. A guinea-pig, the urine of which gave no trace of lithium, had three grains of chloride of lithium, and was killed in two and a half hours. The lithium was found in the cartilage of the hip-joint distinctly but faintly. The blood showed the lithium very distinctly, much more so than in any of the previous experiments. The outer portion of the lens showed lithium, though but slightly. The inner portions of the lens showed more. The vitreous and aqueous humours showed lithium very distinctly.

Experiment 9. A large guinea-pig was given three grains of chloride of lithium, and it was killed in an hour. Lithium was found in the blood, urine, and nails very distinctly; in the cartilage of the hip- and knee-joints very faintly; in the vitreous and aqueous humours of the eye very distinctly. No lithium was found in the lens, not even when half the lens was taken for a single experiment. The stomach contained food.

Experiment 10. Another guinea-pig was killed an hour after the same dose. The lithium was found strongly in the blood, bile, liver, and kidney. Traces occurred in the brain and in the cartilage of the hip-joint. It was present distinctly in the humours of the eye and in the lens. The difference between the inner and outer part of the lens was very marked. The second eye was not examined for more than fourteen hours after the first eye. After this time the centre of the lens contained as much lithium as the outer part did. The stomach contained water.

Experiment 11. A young guinea-pig, fasting, was given three grains of chloride of lithium, and thirty-two minutes afterwards it was killed. Lithium showed faintly in the cartilage of the hip-joint; very distinctly in the humours of the eye; distinctly in the outer part of the lens, very faintly in the inner part; nearly the whole of the inner part had to be burnt to give the appearance. Lithium was very distinct in the blood, and remarkably so in the nails.

Experiment 12. Another young guinea-pig, fed in the same way, and bought at the same place as the two former, was killed without taking any lithia. No lithium was found anywhere. The whole of the spleen, one kidney, and one lens were incinerated, and each ash was used for a single experiment, and in no instance was lithium found. There was no lithium in the cartilage of the hip-joint, nor in the blood, nor in the nails.

Experiment 13. A very young and small guinea-pig that had been kept fasting for thirty-six hours, was given three grains of chloride of lithium, and it was killed in half an hour, the urine having been previously examined, and no lithium found in it. Very much lithium was found in the blood and in the urine; very slight traces in the cartilage and in the brain. The lens showed no lithium when incinerated entire, but the aqueous extract of the lens showed minute traces of lithium.

Experiment 14. An old guinea-pig, also fasting for about thirty-six hours, was given the same quantity of chloride of lithium, and was also killed in half an hour. No lithium could be detected before the dose in the urine, nor in the toe-nail of one leg. After taking the lithium, the animal was wrapped up in a cloth, the leg only being left out, to prevent it from licking the toe; after death, the nails of this leg showed that some lithium was there. The sciatic nerve showed traces of lithium. The cartilage of the hip-joint, when touched with red-hot wire, showed no lithium, but scrapings from the surface showed traces of lithium. The humours of the eye showed traces of lithium, but the lens showed no lithium even in the watery extract. The brain showed only exceedingly faint traces of lithium. The stomach was almost completely empty.

Experiment 15. A guinea-pig was kept fasting for twenty-four hours; it was then given three grains of chloride of lithium, and it was killed in a quarter of an hour. Lithium was found in the bile, liver, kidney, and blood very distinctly; very faintly in the brain and in the cartilage of the hip-joint, and in the humours of the eye. None was found in the lens. The stomach contained only some water, no solid food.

Experiment 16. Three fresh guinea-pigs were taken, one was killed without taking any lithium. The urine showed no lithium in one drop, but the ash of the urine showed traces of lithium. No lithium could be detected in any of the organs, not even by treating the ash of the kidney with sulphuric acid and alcohol.

The two remaining animals were each given one quarter of a grain of chloride of lithium.

The first was killed in five and a quarter hours afterwards. All the organs, except the lens of the eye, showed lithium by simply touching them with the red-hot wire. The urine and the bile showed the lithium very distinctly. The blood showed lithium faintly. The vitreous and aqueous humours showed traces of lithium. An aqueous extract of the lens showed no lithium. The animal was a large and old one, and the stomach was nearly empty.

The second was killed twenty-four hours after one quarter of a grain.

None of the organs showed any lithium by simply touching them with a red-hot wire. The ash of the kidney showed traces of lithium, and so did the ash of part of the liver. No lithium could be detected either in the vitreous and aqueous humour or in the lens; the urine and the bile showed lithium in one drop, but only faintly. Possibly the lithium had not been absorbed in this case. The state of the stomach, as regards food, was not recorded.

It follows from these experiments, that when no lithium is taken no lithium can be found in the different textures, but that even in a quarter of an hour three grains of chloride of lithium given on an empty stomach may diffuse into the cartilage of the hip-joint and into the aqueous humour of the eye. In very young and very small guinea-pigs the same

quantity of lithium in thirty or thirty-two minutes may give traces of lithium in the lens; but in an old animal in this time it will have got no further than the aqueous humour. If the stomach be empty, in an hour the lithium may be very evident in the outer part of the lens, and very faintly in the inner part; but if the stomach be full of food, the lithium does not in an hour reach the lens. Even in two hours and a half lithium may be more marked in the outer than in the inner part of the lens. In four hours the lithium may be in every part of the lens, but less evidence of its presence will be obtained there than from the humours of the eye. In eight hours even, the centre of the lens may show less than the outer part. In twenty-six hours the diffusion had taken place equally through every part of the lens. Even one quarter of a grain in twenty-four hours showed lithium everywhere except in the lens.

Experiment 17. To endeavour to determine the different rate of absorption and excretion in young and old animals, four guinea-pigs were taken; two were young, and two were old. The four, after fasting for fifteen hours, were each given two grains of chloride of lithium. Two of them, one young and one old, were killed in six hours.

The young animal showed lithium distinctly in the outer and inner part of the lens, and also in the cartilage of the hip-joint, when touched with a red-hot wire. The stomach was about half full of food.

The old one showed lithium distinctly in the outer part of the lens, but scarcely the faintest trace in the inner part. The cartilage of the hip-joint showed lithium quite as distinctly as the cartilage of the young pig.

The other two guinea-pigs were kept. After forty-eight hours, the urine of both showed lithium very distinctly in one drop. Six days afterwards, the urine of the young animal still showed lithium faintly in each drop. The urine of the old one found in the bladder after its death showed lithium faintly in each drop.

Both were killed on the sixth day, and no lithium could be detected in the alcoholic extracts of the kidneys, livers, or lenses of either.

A short series of experiments were made with the view of determining the rate at which the salts of lithium diffuse into the textures when the lithium is injected into the skin instead of passing through the stomach.

Three grains of chloride of lithium in solution were injected into the skin of the back of the neck of a guinea-pig, and the animal was killed in twenty-four minutes.

The urine, bile, kidney, and liver showed lithium very distinctly. The cartilage of the hip-joint showed lithium distinctly when touched with a red-hot wire. The aqueous humour showed lithium very distinctly, but the lens, when washed, showed only a very minute trace ot lithium when the entire lens was taken at one time on the wire. The toe-nails showed lithium very distinctly.

Another had three grains injected under the skin of the neck, and it was killed in ten minutes.

The humours of the eye showed lithium distinctly, but the aqueous humour showed decidedly more than the vitreous humour. The incinerated aqueous extract of the lenses showed lithium very faintly. The large nerves of the leg also showed lithium very faintly.

Another guinea-pig had a grain and a half of chloride of lithium injected under the skin of the neck, and in five minutes it was killed. The aqueous humour showed lithium distinctly. The vitreous humour showed none. The blood and bile showed lithium very distinctly. The kidney and urine showed lithium faintly, and the liver very faintly.

In another pig three grains of chloride of lithium were injected into the skin of the neck, and the animal was killed in four minutes.

The blood and the bile showed lithium very distinctly; the blood showed it rather more than the bile. The bladder contained only a few drops of urine, which showed lithium distinctly. The kidney showed lithium fairly well. The liver showed the lithium only very faintly, and in some parts not at all.

The aqueous humour showed lithium distinctly. The vitreous humour showed no lithium.

So that, when injected under the skin,

3 grains in twenty-four minutes gave lithium in the lens and everywhere.

3 grains in ten minutes gave lithium in the lens and everywhere.

$1\frac{1}{2}$ grain in five minutes gave lithium in the aqueous humour and in the bile.

3 grains in four minutes gave lithium in the aqueous humour and in the bile.

3. *Experiments on the Rate of Passage of the Lithium out of the Textures.*

Experiment 18. Five guinea-pigs were given two grains of chloride of lithium each. They were killed at different periods; the first in six hours. The smallest particle of the lens showed the lithium very distinctly; a decided difference, however, was detectable between the inner and the outer part. The cartilage of the hip-joint showed lithium very distinctly when touched with the red-hot wire. All the organs and the blood showed lithium very abundantly. The stomach contained very little solid food, but was half full of liquid. The second and third were killed in twenty-four hours. The lenses of both showed the lithium very distinctly; no difference was perceptible between the inner and the outer part. The cartilage of the hip-joint showed no lithium when touched with the red-hot wire; but a small portion taken off the surface showed lithium distinctly.

The fourth guinea-pig was killed in forty-eight hours. The lens showed

lithium very distinctly. A small piece taken from the cartilage of the hip-joint showed only traces of lithium.

The fifth was killed in ninety-six hours. The lens showed no lithium even when a considerable proportion of it was taken for one experiment. The aqueous extract of half one lens showed no lithium. A small portion of the cartilage of the hip-joint showed no lithium. The urine showed lithium very distinctly even in one drop.

Experiment 19. Six fresh guinea-pigs were taken. The first was killed and examined, having had no lithium. The two lenses, incinerated and treated with sulphuric acid and alcohol, showed no lithium. The ash of the kidney showed no lithium directly, but when treated with sulphuric acid and alcohol, showed a distinct trace of lithium.

The five others were given each one grain of chloride of lithium.

The first was killed five and a half hours after the dose. The cartilage of the hip-joint showed lithium faintly when merely touched with a red-hot wire. The lens showed lithium distinctly in the outer part, scarcely a trace in the inner part. The vitreous and aqueous humours showed lithium very distinctly. The stomach was quite full.

The second was killed twenty-four and a half hours after the lithium was taken. The cartilage of the hip-joint showed no lithium even in a small particle scraped off the surface. The lens still showed lithium distinctly, though less so than in the first; no difference was perceptible between the inner and the outer portion.

The third was killed in forty-eight hours. The lens showed no lithium in a small particle taken on a loop of the wire. The aqueous extract of the lens showed only faint traces of lithium. The urine showed lithium very distinctly in a single drop.

The fourth was killed in seventy-two and a half hours. The lens showed no lithium when the ash was treated with sulphuric acid and alcohol. The ash of the kidney showed no lithium directly, but when treated with sulphuric acid and alcohol, showed traces of lithium. The urine still showed lithium distinctly in one drop.

The fifth guinea-pig: on the seventh day after the dose, the urine showed lithium in one drop; ninth day, still faint traces of lithium in the urine; eleventh day, urine directly shows no lithium, but the ash still shows faint traces; thirteenth day, ash of urine shows no lithium, but alcoholic extract shows lithium distinctly; fourteenth day the same; sixteenth day the same; thirty-sixth day, when killed, no lithium could be detected in the bones, nerves, lens, or vitreous or aqueous humours, nor in the urine, kidney, or liver.

Experiment 20. Two guinea-pigs, in the urine of which no lithium could be detected, were given each half a grain of chloride of lithium.

In three hours and fifty minutes afterwards one was killed. The cartilage of the hip-joint showed no lithium when simply touched with a red-

hot wire. Scrapings from the surface of the cartilage showed faint traces of lithium. The sciatic nerve, humours of the eye, and the brain showed faint traces. The muscles of the thigh showed the lithium much more distinctly than the sciatic nerve. The lens showed lithium very distinctly in the aqueous extract, but not otherwise. The blood and bile were very rich in lithium. The stomach was moderately full of food.

The other animal, which was given half a grain, was kept until the lithium ceased to appear in the urine.

Fourth day. Lithium distinctly in the urine.

Tenth day. Urine showed exceedingly minute traces of lithium.

Eleventh day. Still traces.

Thirteenth day. Urine shows no lithium in the quantity adhering to the wire.

Fourteenth day. Still lithium in the alcoholic extract.

Twenty-seventh day. Still traces of lithium.

Thirtieth day. The animal was found dead.

The ash of the urine found in the bladder (about a quarter of an ounce) showed no lithium. The alcoholic extract of the ash showed lithium faintly. The alcoholic extract of the ash of one kidney showed no lithium. And the alcoholic extract of the two lenses showed no lithium.

Experiment 21. Two guinea-pigs, the urine of which contained no lithium, were each given one quarter of a grain of chloride of lithium.

One was killed in four hours and thirty-five minutes. Lithium was found very faintly in the spleen, very distinctly in the blood, in the urine, and in the bile. Faintly in the sciatic nerve and in the brain. Very faintly in the scrapings of the cartilage. Pretty distinctly in the vitreous and aqueous humours, but very faintly in the aqueous extract of the lens. The stomach was moderately full.

The other was kept until the lithium ceased to appear in the urine.

Second day. Lithium very distinctly.

Fourth day. Minute traces of lithium.

Sixth day. A drop or two of urine shows no lithium, but on evaporating and incinerating one-twelfth of an ounce, the ash shows lithium very distinctly.

Seventh day. Lithium still distinct in the ash.

Eighth day. Still in the ash.

Tenth day. Ash of urine shows only the merest trace.

Eleventh day. Ash of urine shows no lithium; but when treated with sulphuric acid and alcohol, lithium is still distinct.

Thirteenth day. Still lithium in one quarter of an ounce.

Fourteenth day. Alcoholic extract from one-eighth of an ounce shows no lithium.

Sixteenth day. The animal was killed. The fluids and organs were incinerated, the ash treated with sulphuric acid, excess of acid driven off

and the dry residue extracted with absolute alcohol, alcoholic extract evaporated, and dry residue tested. The two lenses gave extremely feeble traces of lithium. One-eighth of an ounce of urine and bile gave traces of lithium. Ninety grains of liver gave traces. One quarter of an ounce of blood gave no lithium. An entire kidney, weighing ninety grains, distinctly contained lithium.

Experiment 22. Two guinea-pigs, the hair and nails of which showed no lithium, were given each three grains of chloride of lithium.

In the first, in two hours no lithium was in the hair. In four hours lithium was in the hair of the belly, but scarcely perceptible in the hair of the head. In twenty-four hours it was very distinct in the hair of the belly and the head, and in the nails. For five days it was detected in each drop of the urine. Ten days afterwards the urine showed lithium very distinctly; only after thirty-two days was lithium absent from a few drops of the urine. The thirty-third day after the dose the animal was killed. No lithium was found in the bile, liver, blood, lens, kidneys, spleen, or other parts, by simply taking a small piece of the organ on a red-hot wire. The evaporated aqueous extract of the two lenses showed no trace of lithium; when, however, the two kidneys were incinerated, the ash treated with sulphuric acid, the resulting sulphates exhausted with absolute alcohol and the alcoholic extract evaporated to dryness, lithium was easily detected in the residue. A portion of the liver, treated in the same manner, also yielded lithium.

The second guinea-pig gave traces of lithium in the urine when one-eighth of an ounce was evaporated and treated with sulphuric acid and alcohol, thirty-nine days after the lithium was taken.

It follows from these experiments on the rate of passage of lithium into and out of the body, that—

With three grains of chloride of lithium, a young guinea-pig in half an hour had lithium in the watery extract of the lens. An old guinea-pig in the same time had no lithium in the lens.

With two grains, a young guinea-pig in six hours had lithium distinctly in all parts of the lens. An old guinea-pig had in the same time scarcely any lithium in the inner part, but some in the outer part of the lens.

With the same quantity, in six days neither a young nor an old guinea-pig gave any trace of lithium in the alcoholic extract of the kidney, liver, or lenses.

When two grains of chloride of lithium were taken, after six hours the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours no difference in the different parts of the lens was detectable. In forty-eight hours still no difference was observed. In ninety-six hours (four days) no lithium was detectable in the lens or in a cartilage of a joint; still the urine showed lithium very distinctly even in one drop.

After one grain of chloride of lithium, in five hours and a half the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours and a half there was no difference throughout the lens. In forty-eight hours the watery extract of the lens showed faint traces of lithium. In seventy-two hours and a half (three days) the alcoholic extract of the lens showed no lithium. The urine still showed lithium distinctly in one drop, and continued to do so for seventeen days in the alcoholic extract.

After a quarter of a grain, in five hours and thirty-five minutes lithium was distinct in the vitreous and aqueous humours, and very faintly in the lens. After sixteen days, the minutest traces of lithium were detected in the lens, the liver, and the kidneys, but no trace could be found in the blood. (This animal had perhaps somehow eaten the minutest quantity of lithia in the food*.)

After half a grain of chloride of lithium, in three hours and fifty minutes traces of lithium could be found in the lens, and for thirty-seven or thirty-eight days traces of lithium could be found in the urine.

After three grains of chloride of lithium, in four hours lithium was in the hair of the belly, and for thirty-two days the urine showed lithium very distinctly. The thirty-third day after the lithium the lens was found to contain minute traces of lithium, and after thirty-nine days the lithium was in the alcoholic extract of the urine.

4. *Experiments on the Rate of Passage of Lithium through the Human Body, and into and out of the Crystalline Lens.*

Experiment 1. A man took ten grains of carbonate of lithia dissolved in water, four hours after his midday food.

In five minutes no lithium could be detected in the urine.

In ten minutes lithium was evident.

In eighteen hours it was present in the nails of the hands and feet, and in the hair of the beard and body; apparently most where there was most perspiration. No lithium could be found in the hair of the head or whiskers.

In forty-two hours very perceptible.

In sixty-six hours another dose of ten grains was taken.

In ninety hours lithium was detectable in the hair of the head.

For three days after the second dose it was perceptible in one drop of the urine, but rather doubtful in the hair and in the nails.

For six days after the second dose lithium was detectable in the urine.

For eight days after, no lithium could be detected when the eighth of an ounce of urine was evaporated.

* The skin of guinea-pigs throws off lithium, and it collects on the hair and nails, so that it is possible for the animal to redose itself with lithium from its own body, and thus to keep lithium passing in and out of the textures much longer than if a single dose only were taken.

Twelve days afterwards, though no lithium was in the urine or the hair of the head or whiskers, it was detectable in the hair of the body.

Experiment 2. The same man three hours after breakfast took ten grains of carbonate of lithia.

In five minutes lithium was just perceptible in the urine.

In ten minutes extremely distinct in one drop of the urine.

In twenty-four hours very distinct in the urine.

Fourth day. Traces of lithium when the urine was concentrated by evaporation.

Fifth day. Less perceptible in evaporated residue.

Sixth day. No lithium could be detected in evaporated urine.

Experiment 3. The same man took ten grains of carbonate of lithia after fasting for seven hours. The urine was passed every second minute after taking the lithia.

Second, fourth, and sixth minute, no lithium.

Eighth minute, traces of lithium very slight.

Tenth minute, lithium distinctly present.

Third day afterwards lithium very distinct.

Fourth day. Lithium faintly found in each drop of urine.

Fifth day. Lithium very faint in each drop.

Sixth day. Only the merest trace.

Seventh day. No lithium in the eighth of an ounce evaporated to a few drops.

Eighteenth day. Two ounces of urine incinerated, and the ash heated with sulphuric acid and alcohol, showed no lithium.

Twenty-first day. Nails of the hands and feet still show lithium.

Experiment 4. The same man, two hours and a half after a little food, took ten grains of chloride of lithium: one nail on the hand and one on the foot were varnished before taking the lithium.

Second and fourth minute afterwards no lithium was in the urine.

Sixth minute, traces of lithium.

Eighth minute, distinctly present.

Tenth minute, very distinctly.

Twenty-five hours afterwards none of the nails showed any lithium.

Forty-four hours. Scrapings of the unvarnished nails on the hands and feet showed lithium distinctly. The further the scraping was carried the less lithium was found. The scrapings of the varnished nail of the hand shows only traces of lithium. The varnished nail of the foot has no lithium. A small particle of the skin from the hand or foot shows lithium distinctly. Perspiration shows lithium distinctly. The hair of the head or whiskers shows no lithium.

Third day. Nails the same as yesterday. The unvarnished nails show lithium; the varnished, none. Urine shows lithium most distinctly.

Fourth day. Urine shows lithium in one drop.

Fifth day. Urine still shows minute traces of lithium in one drop.

Sixth day. Urine shows no lithium. Ash of the urine shows faint traces of lithium.

Seventh day. Ash of urine shows no lithium. Alcoholic extract shows traces.

Eighth day. Alcoholic extract from one ounce of urine still shows traces of lithium.

Ninth day. Alcoholic extract from one ounce of urine shows no lithium.

Experiment 5. A boy, aged sixteen years, took five grains of chloride of lithium, and the urine was passed every second minute. Half an hour previously he had eaten some bread and butter. No lithium could be detected in the urine previous to the taking of the dose.

Second minute, no lithium in the urine.

Fifth minute, none.

Ninth minute, none.

Tenth minute, very faint traces of lithium.

Thirteenth minute, lithium very distinctly present.

After twenty-four hours lithium still very distinct.

Second, third, fourth, and fifth day. Still very distinct.

Seventh day. No lithium was found in the evaporated residue. The ash of the residue shows very slight traces.

Eighth day. The alcoholic extract from one ounce of urine shows no lithium.

Experiment 6. The same boy had his hair, nails, and urine examined, and no lithium was found.

Five grains of carbonate of lithia was then given to him.

In two minutes, five minutes, ten minutes, no lithium was found in the urine.

In twenty minutes lithium was distinctly present.

In eighteen hours the lithium was found in the nails, none in the hair of the head.

In thirty-two hours, still none in the hair of the head. Very distinctly in the root and tip of the nails.

Another five grains of carbonate of lithia was then given.

In nineteen hours lithium was detected in the hair of the head.

In four days a drop of the urine showed lithium very distinctly, as did the hair and nails.

In five days the same.

In seven days one drop of the urine shows no lithium, but if the urine is slightly concentrated by evaporation, lithium is still perceptible.

In eight days one-eighth of an ounce evaporated still shows slight traces.

In nine days no lithium in one-eighth of an ounce of urine.

Experiment 7. The same boy took five grains of carbonate of lithia, but he omitted previously to empty his bladder.

In five minutes, lithium not yet detectable in the urine,

In ten minutes lithium very distinctly present in one drop of the urine.

Four days afterwards traces of lithium still in the urine.

Five days afterwards slight traces in one-eighth of an ounce of urine when evaporated.

In six days afterwards no lithium perceptible in the urine.

Experiment 8. Twenty-five grains of chloride of lithium were dissolved in one gallon of water, and the feet and ankles of a man were kept in the solution for two hours; at the end of this time the urine was passed and examined for lithium, and no trace could be found in the aqueous extract of the ash of one quarter of an ounce of urine.

These experiments agree very closely with some which I made many years since on a full-grown German who had an open bladder, admitting the urine to be caught as it came from the kidneys.

Feb. 24, 1852. At 8.45 A.M. he took two cups of black coffee and nothing else.

9.30 to 9.50. Urine was caught, and it contained no trace of iron.

9.50. Protosulphate of iron, 6·7 grains, free from persulphate was taken in two ounces of distilled water.

9.55. Urine caught and contained no iron.

10.	No iron as peroxide.	Present as protoxide.
10.5.	Slightest trace of peroxide.	Protoxide distinct.
10.10.	Slightest trace of peroxide.	Protoxide distinct.
10.20.	Slightest trace of peroxide.	Protoxide less distinct.
10.30.	,, ,,	Protoxide less distinct.
10.40.	,, ,,	Slightest trace of protoxide.
11.	,, ,,	No trace.
11.10.	,, ,,	No trace.

Feb. 26. The same patient. At 8 A.M. two cups of black coffee and nothing else.

10.20 to 10.30. Urine caught and no trace of iron found.

10.30. Sulphate of protoxide of iron four grains, given in one ounce of distilled water.

10.31. No trace of iron in the urine.

10.34. No trace.

10.35. No trace.

10.36. No trace.

10.37. Slightest trace of protoxide of iron in the urine. No peroxide.

10.39. No trace.

10.40. No trace.

March 2nd. The same patient. At 8 A.M. two cups of black coffee and nothing else.

9.34 to 9.40. Urine collected and no trace of iron found.

9.40. Sulphate of protoxide given, seven grains in two ounces of distilled water.

9.42½. None.

9.45. None.
9.47½. None.
9.50½. A trace.
9.52½. A trace.
9.55. Good.
9.57½. Doubtful.
10. Doubtful.
10.5. More distinct.
10.10. Doubtful.
10.15. Doubtful.

March 19. At 8 A.M. two cups of black coffee without milk, nothing else taken.

Urine from 9.50 to 9.56 collected; contained no iodine. One grain of iodide of potassium dissolved in one ounce of water was then taken.

9·58. No iodine.
9.59. No iodine.
10. None.
10.1. None.
10.2. None.
10.3. None.
10·4. None.
10·5. None.
10.6. None.
10.8. Trace of iodine.
10.10. Very marked iodine.
10.15. Very marked.

So that one grain of iodide of potassium in one ounce of water was detected in the urine in twelve minutes, and was very marked in fourteen minutes. Iron was detected once in seven minutes and twice in ten minutes, and it was very distinct in fifteen minutes.

Professor Mulder also made many experiments on this patient, but I am unable to find any account of his results.

In the 'Medical Gazette' for 1845, pp. 363 & 410, Mr. Erichsen gives some experiments he made on a boy of thirteen who had an open bladder. He states that twenty grains of ferrocyanide of potassium were detected in one minute in the urine. The stomach was fasting, and the salt was dissolved in three ounces of water. Forty grains taken three quarters of an hour after a full meal were only detected after thirty-nine minutes.

Forty grains in four ounces of water were twice detected in two minutes, and no trace could be found after twenty-four hours; once in two minutes and a half; once in six minutes and a half; once in fourteen minutes; once in twenty-seven minutes; and once in thirty-nine minutes.

Twenty grains of ferrocyanide he once detected for twenty-eight hours.

It follows from these experiments that ten grains of carbonate or chloride of lithium, taken two and a half, three, or four hours after food by a man,

require between five and ten minutes to pass from the stomach to the urine, and this quantity of carbonate or chloride of lithium will continue to produce traces of lithium in the urine from six to seven, or even eight days.

Five grains of chloride or carbonate of lithia, taken shortly after food by a boy, gives no appearance in the urine until from ten to twenty minutes, and this quantity continues to pass out for five, seven, or eight days.

Experiments made by the ordinary mode of analysis showed that

Four grains of sulphate of the protoxide of iron, taken almost fasting by a man, gave a trace in the urine in seven minutes.

Seven grains gave distinct appearance in ten minutes and ten minutes and a half.

One grain of iodide of potassium, taken by the same man fasting, appeared in the urine in twelve minutes.

Experiments on the Rate of Passage of Lithium into and out of the Crystalline Lens.

Through the kindness of Mr. Bowman and Mr. Critchett at the Moorfields Ophthalmic Hospital, lithia water, containing variable quantities of lithia, was given to different patients about to be operated on for cataract.

Experiment 1. The hard cataracts from two patients who had taken no lithia water were examined; no trace of lithium could be found in either lens.

Experiment 2. The hard cataracts from two other patients who had no lithia water were examined; an aqueous extract of each lens was made; one showed the most excessively feeble lithium line; the other lens did not give the slightest indication.

Experiment 3. The hard cataracts of two other patients who had taken no lithia water were examined; the alcoholic extract of the ash, after treatment with sulphuric acid, showed no lithium in either lens.

The lens of a third patient was examined when no lithia water had been taken, and the alcoholic extract showed no lithium.

Experiment 4. The lens of a man aged seventy was extracted twenty-five minutes after he had taken twenty grains of carbonate of lithia in water on an empty stomach; no lithium could be detected in the lens.

Experiment 5. A woman, æt. sixty-four, at 9 A.M. took twenty grains of carbonate of lithia in water; both lenses were extracted at $11\frac{1}{2}$ A.M. the same day. Neither of the lenses showed any lithium when touched with a red-hot wire, but the aqueous extract of one lens showed lithium faintly, and the aqueous extract of the other lens showed lithium distinctly.

Experiment 6. An eye was removed three hours after twenty grains of carbonate of lithia had been taken; the lens was removed half an hour afterwards, and on examination every portion of the lens contained lithium. The circulation through the eye had been healthy, and the lens itself was clear.

Experiment 7. The soft lens of a girl aged fourteen was examined after

ten grains of carbonate of lithia in water had been taken five hours before the operation, and the same quantity four hours before extraction.

The smallest fraction of the lens showed the lithium distinctly.

Experiment 8. Another patient with two soft cataracts took twenty grains of carbonate of lithia seven hours before one operation; but the capsule of the lens had been previously broken, so as to expose the cataract to the aqueous humour.

Lithium was found very distinctly even in the smallest particle of the cataract.

Four days after the first operation the capsule of the other lens was broken, so as to expose the cataract to the aqueous humour; and seven days after the first operation the second operation was performed.

In this cataract not the slightest trace of lithium could be found.

A woman with diseased heart drank some lithia water, containing fifteen grains of citrate of lithia, thirty-six hours before her death; and six hours before death she drank the same quantity.

After death the crystalline lens, the blood, and the cartilage of one joint were examined for lithium.

The cartilage showed lithium very distinctly; the blood showed lithium very faintly; and when the entire lens was taken for a single examination, the faintest possible indications of lithium were obtained.

Another patient five and a half hours before death drank lithia water containing ten grains of carbonate of lithia.

After death the cartilage of one joint and the crystalline lens were examined.

The cartilage showed lithium very distinctly. When half the lens was taken for a single analysis, only very faint traces of lithium could be found.

When no lithia had been taken, seven cataracts were examined most carefully, and one only showed an exceedingly feeble trace of lithium.

When twenty grains of carbonate of lithia were taken twenty-five minutes before the operation, the lens showed no lithium; the same quantity taken two and a half hours before, showed lithium in the watery extract; three and a half hours before, showed lithium in each particle; between four and five hours before, the same; seven hours before, the same; seven days before, not the slightest trace of lithium.

Thirty grains of carbonate of lithia, taken between six and thirty-six hours before death, showed the faintest indications of lithium in the lens.

Ten grains of carbonate of lithia taken five and a half hours before death, gave only faint traces of lithium in the lens.

On the Passage of Solutions of Lithium through the Textures after death.

A sheep's eye was examined after death and no lithium could be detected in any part. Two other eyes were placed in a solution of chloride of lithium containing one grain to one litre of water. Twenty-three hours afterwards the lithium was found to have penetrated through the entire

eye. There was, however, a perceptible difference between the amount of lithium in the inner and outer part of the lenses.

Two other sheep's eyes had a small portion of the cornea in front and the sclerotic removed at the back; they were then placed in a moderately strong solution of chloride of lithium, and the aqueous humour was examined from time to time.

After eighteen hours the aqueous humour showed lithium distinctly, and when the lens was extracted the lithium was found throughout its substance.

Two other eyes were placed whole in a solution containing one-tenth of a grain of chloride of lithium to one litre of water.

In twenty-four hours the lithium had penetrated the entire eye. No difference was perceptible in different parts of the lens.

The rate at which a solution of chloride of lithium diffused through the stomach of a fresh-killed guinea-pig which had taken no lithium was determined.

A solution of one grain of chloride of lithium in twenty grains of water was put into the stomach, and it was hung up so that the solution gravitated to the lowest part. The outer side of the stomach opposite the solution was touched from time to time with a loop of platinum wire, which was afterwards tested for lithium.

In first minute. No lithium came through the stomach.

In second minute. No lithium.

In third minute. No lithium.

In fourth minute. No lithium.

In fifth minute. No lithium.

In sixth minute. Traces of lithium.

In seventh minute. Traces of lithium.

In eighth minute. Lithium was very distinct.

The stomach of another guinea-pig was filled with a solution of lithium containing one grain of lithium in about half an ounce of water. The stomach was entirely filled and laid flat on a plate. The ends of the stomach and round the side showed lithium coming through in four minutes. The upper part of the stomach showed the lithium coming through in fifteen minutes.

5. *On the Presence of Lithium in Solid and Liquid Food.*

An ounce of each substance was taken. It was dried or evaporated, and incinerated carefully at a low red heat in a muffle on a platinum tray. The ash was tested for lithium first by taking a small fraction on a loop of a platinum wire into the flame of the spectroscope. When no lithium was thus detected, the ash was treated with sulphuric acid, and heated to expel the excess of acid; the dry residue was extracted with absolute alcohol, the solution filtered, evaporated to dryness, and the residue taken up in a drop of water and tested by the spectroscope.

Potatoes.	In ash direct.	In alcoholic extract.
No. 1.	No lithium.	No lithium.
2.	,,	,,
3.	,,	,,
4.	,,	Lithium distinctly.
5.	,,	No lithium.
Apples.		
No. 1.	,,	,,
2.	,,	,,
3.	,,	Lithium distinctly.
4.	,,	Trace of lithium.
Carrots.		
No. 1.	,,	No lithium.
2.	,,	,,
Bread.		
No. 1.	,,	Slight traces of lithium.
2.	,,	Traces of lithium.
3.	,,	Lithium distinctly.
Savoy Cabbage.		
No. 1.	Lithium distinctly.	
2.	No lithium.	Lithium shown distinctly.
Tea.		
No. 1.	,,	,,
2.	,,	Lithium very faintly.
3.	,,	Lithium very distinctly.
4.	,,	Faintly.
5.	,,	,,
6.	,,	,,
7.	,,	No lithium.
8.	,,	Faintly.
9.	,,	No lithium.
10.	,,	Very distinctly.
Coffee.		
No. 1.	,,	Very faintly.
2.	,,	,,
3.	,,	No lithium.
4.	,,	Lithium distinctly.
5.	,,	Very distinctly.

Wines: in almost all cases the ash gave direct evidence of the presence of lithium.

Port Wines.		
No. 1.	Small traces of lithium.	
2.	Faintly.	
3.	Very faintly.	Lithium exceedingly distinct.

Port wines.	In ash direct.	In alcoholic extract.
4.	Very faintly.	Lithium exceedingly distinct.
5.	No lithium.	Very faintly.
6.	,,	Very distinctly.
Sherry.		
No. 1.	Lithium extremely brightly.	
2.	Faintly.	
3.	Exceedingly faintly.	
4.	Very faintly.	
5.	Faintly.	
6.	Distinctly.	
French Wines.		
No. 1. (red).	Lithium very distinctly.	
2. (white).	Extremely distinctly.	
3. (champagne).	Very brightly.	
4. (,,).	,,	
Rhine Wines.		
No. 1.	Lithium exceedingly faintly.	
2.	,,	
3.	Lithium distinctly.	
4.	Very faintly.	
5.	Distinctly.	
6.	Faintly.	
7.	Distinctly.	
8.	Faintly.	
Ale.		
No. 1.	No lithium.	Lithium faintly.
2.	,,	No lithium.
3.	,,	Lithium faintly.
Porter.		
No. 1.	,,	,,
2.	,,	No lithium.
3.	,,	Lithium distinctly.

In the Philosophical Magazine, vol. xx. Messrs. A. and F. Dupré gave the spectrum analysis of London waters. All the different waters examined gave lithium. The shallow waters appear to be richer in lithium than the deep-well waters. The different waters examined were: Thames water at high and low tide at Westminster Bridge; the water from Chelsea and Lambeth Water-Companies; New River water; Duck Island well, in St. James's Park; Pump in Lincoln's-Inn. These were above the London clay.

Burnett's Distillery and Whitbread's Brewery: from the sand above the Chalk.

Guy's Hospital well and Trafalgar Square well: from the Chalk.

	In ash direct.	In alcoholic extract.
Entire sheep's kidney.	No lithium.	Very faint traces.
One ounce of kidney.	,,	No lithium.
One ounce of mutton.	,,	,,
One ounce of beef.	,,	,,

It appears from these experiments that

Potatoes	showed	lithium	once in five trials.
Apples	,,	,,	twice in four trials.
Carrots	,,	no lithium in two trials.	
Bread	,,	lithium thrice in three trials.	
Cabbage	,,	,,	twice in two trials.
Tea	,,	,,	eight times in ten trials.
Coffee	,,	,,	four times in five trials.
Port wine	,,	,,	six times in six trials.
Sherry	,,	,,	six times in six trials.
French wine,		,,	four times in four trials.
Rhine wine	,,	,,	eight times in eight trials.
Ale	,,	,,	twice in three trials (traces).
Porter	,,	,,	twice in three trials (traces).

Mutton, beef, and sheep's kidney showed no lithium: one kidney had a slight trace of lithium.

I hope in a future paper, with the help of Dr. Dupré, to show that Thallium, Rubidium, and Cæsium, by spectrum analysis, can be traced even into the crystalline lens, and to determine the rate at which they pass in, if not out of, the textures; and by other means we shall endeavour to trace the passage of other crystalloids throughout the textures.

Conclusions.

1. *On the Rate of Passage of Solutions of Lithium into the Textures of Animals.*

In guinea-pigs, even in a quarter of an hour after three grains of chloride of lithium are taken into the stomach, the lithium may be found not only in all the vascular textures, but even in the cartilage of the hip-joint, and in the humours of the eye. If the same quantity is injected into the skin, in ten minutes it can be detected in the lens and everywhere; and even in four minutes the lithium may be detected everywhere except in the lens.

In half an hour after the same quantity is taken into the stomach, lithium may be found in the crystalline lens.

After it has been taken eight hours, it may not have passed completely into the inner part of the lens.

In twenty-six hours it will be found in every part of the lens.

When half a grain only of chloride of lithium was taken, in less than

four hours traces were found in the lens. And even when only a quarter of a grain was taken, faint traces of lithium were found in five and a half hours.

2. *On the Rate of Passage of Solutions of Lithium out of the Textures of Animals.*

After two grains of chloride of lithium, in six days neither a young nor an old guinea-pig gave any lithium in the kidney, liver, or lenses.

After two grains, in four days no lithium could be found in the lens, nor in the cartilage of a joint.

After one grain, in three days the alcoholic extract of the lens showed no lithium.

After a quarter of a grain of chloride of lithium, in sixteen days the minutest traces of lithium were detected in the liver, kidneys, and lens.

After half a grain, for thirty-seven or thirty-eight days traces of lithium could be found in the urine.

After three grains, traces were found in the lens for thirty-three days, and for thirty-nine days, the smallest quantity could be found in urine.

The skin of guinea-pigs throws off lithium, and it collects on the hair and nails; so that it is possible for the animal to redose itself with lithia from its own body, and thus to keep lithia passing in and out of the textures much longer than if a single dose only were taken.

3. *On the Rate of Passage of Solutions of Lithium in and out of the Human Body.*

In ten-grain doses, lithium may be found in the urine in from five to ten minutes, and continue to pass out for six or seven days.

In five-grain doses it may be in the urine in from ten to twenty minutes, and continue to pass out even for eight days.

In twenty-grain doses, it may be found in small quantity in the crystalline lens in two and a half hours, and be present in every particle of the lens in three and a half, five, and seven hours; and no trace of lithium may be detectable in the lens after seven days, when twenty grains of the carbonate of lithia had been taken.

4. *Results of the examination of Solid and Liquid Food.*

Although almost every kind of vegetable food, and almost every fluid which we drink, contains infinitesimal quantities of lithia, yet rarely, if ever, can lithium be detected in any part of the body of man or animals, unless some larger quantity is taken than ordinarily occurs in the food or drink.

Appendix.—Received July 8, 1865.

On the Passage of Chloride of Rubidium into the Textures.

A guinea-pig was given three grains of chloride of rubidium at 11 A.M.

At 6.30 P.M. it was killed. Rubidium was not detectable anywhere; not even satisfactorily in the urine.

Another guinea-pig was given ten grains of chloride of rubidium at 11.20 A.M. At 3 P.M. scarcely any rubidium could be detected in the urine. The following day, at 11 A.M., it was given five grains more. At 2 P.M. rubidium was just detectable in the urine. The next day, at 2 P.M., it was again given five grains, the rubidium being just perceptible in the urine. Twenty-five hours afterwards it was killed.

Extremely minute traces of rubidium were found in the kidney and in the blood; somewhat more, but still very faint traces, in the liver. In the cartilages no rubidium could be found, nor in the aqueous humour of the eye. When the whole lens was incinerated at once the smallest possible trace of rubidium was found. The urine showed traces of rubidium.

An elderly man took nineteen grains of chloride of rubidium four hours before he was operated on for cataract. The most careful search could not find any rubidium in the lens after its removal.

Another patient, with a double cataract, was given twenty grains of chloride of rubidium. One lens was extracted ten hours afterwards, and the other seven days afterwards, but in neither could traces of rubidium be found.

It was found by experiment that $\frac{1}{16,000}$ of a grain of chloride of rubidium in water was detectable by the spectrum analysis. $\frac{1}{8000}$ of a grain in urine could be distinctly observed.

On the Passage of Chloride of Cæsium into the Textures.

Delicacy of the reaction for Cæsium.—One grain of chloride of cæsium in 400 cub. centims. of water just gives the blue cæsium lines in a quantity of solution that can adhere to the loop of a platinum wire which took up 0·05 of solution. The $\frac{1}{125,000}$ part of a grain of chloride of cæsium in water can be detected. If potassium is present in the same solution the test is much less delicate.

In urine, one grain of chloride of cæsium in 200 cub. centims. is the limit of the reaction for a quantity remaining on the loop of the same wire as was previously used. Hence $\frac{1}{62,500}$ of a grain of chloride of cæsium in urine can be detected.

A guinea-pig was given three grains of chloride of cæsium, and twenty hours afterwards another three grains. Twenty hours after the second quantity it was killed. The ash of the urine showed cæsium slightly. No cæsium could be detected in the two lenses taken for one examination; nor in the liquid humours of the eyes. A small portion of the ash of the kidneys and liver showed no cæsium, but aqueous extracts, after concentration, showed cæsium faintly. No cæsium could be detected in the blood, nor in the bile.

A guinea-pig was given six grains of chloride of cæsium, and six grains more nineteen hours afterwards; twenty-four hours after the second dose

it was killed. No cæsium could be found in the lenses, nerves, aqueous humour, blood, or bile. Urine, kidney, and liver showed cæsium slightly in the aqueous extract of the ash.

A guinea-pig was given ten grains of chloride of cæsium, and twenty hours afterwards ten grains more. Twenty-seven hours after the second dose it was killed.

The evaporated and incinerated extract of the two lenses showed the cæsium only faintly. The aqueous humour of the eye showed cæsium faintly. The evaporated and incinerated extract of the two large nerves of the legs showed cæsium pretty distinctly.

On the Passage of Sulphate of Thallium into the Textures.

A rabbit was given one grain of sulphate of thallium. The urine, passed two hours after the first dose, gave the reaction very clearly.

Another rabbit was given three grains of sulphate of thallium, and it was killed twenty-one hours and a half afterwards. This rabbit took no food after the dose of thallium, but the stomach was found completely full of dry food. Thallium was found in the kidneys, liver, and spleen, by simply touching with a red-hot wire and bringing the small quantity of substance adhering to the wire into the flame. The blood, lens, and cartilage showed none in this manner. The aqueous extract, however, of the coagulated blood and lens showed thallium distinctly. The cartilage of the hip could not be thus examined, owing to the small quantity to be got.

Another rabbit was given three grains of sulphate of thallium, and it was killed in six hours and a half. The aqueous extract of the lens showed thallium distinctly.

A guinea-pig was given two grains of sulphate of thallium, and twenty hours afterwards it took two grains more; twenty-two hours after the second dose it was killed. The urine showed thallium only after concentration. Small pieces of the liver, kidney, cartilage of the short ribs, and large nerve of the leg showed thallium distinctly. Humours of the eye showed thallium distinctly. Aqueous extract of the lenses together showed it distinctly. The blood showed no thallium directly, but the aqueous extract of a small quantity of coagulated blood showed the thallium very faintly. The brain showed the thallium also very faintly. The toe-nails showed the thallium very distinctly; and the hair of the belly also showed it very distinctly.

Another guinea-pig was given two grains of sulphate of thallium, and it was killed in six hours. The aqueous extract of the lens showed thallium faintly. The urine showed the thallium distinctly. The aqueous extract of the two large nerves showed no thallium.

On the Passage of Sulphate of Silver into the Textures.

A guinea-pig was given one-eighth of a grain of sulphate of silver. Twenty-three hours afterwards it was given another eighth of a grain.

Twenty-seven hours afterwards a third eighth of a grain was given; and the same dose on the third, fourth, fifth, sixth, seventh, ninth, and tenth days; on the eleventh day the animal died. One grain and a quarter of sulphate of silver in twelve days was taken. The ashes of the liver, kidney, and stomach showed silver fairly, by means of galvanic precipitation of silver or copper. The ash of the bile showed silver rather less distinctly. The ash of the urine showed the silver only very slightly. The ash of the lenses showed only very slight traces of silver, and the ash of the brain showed none.

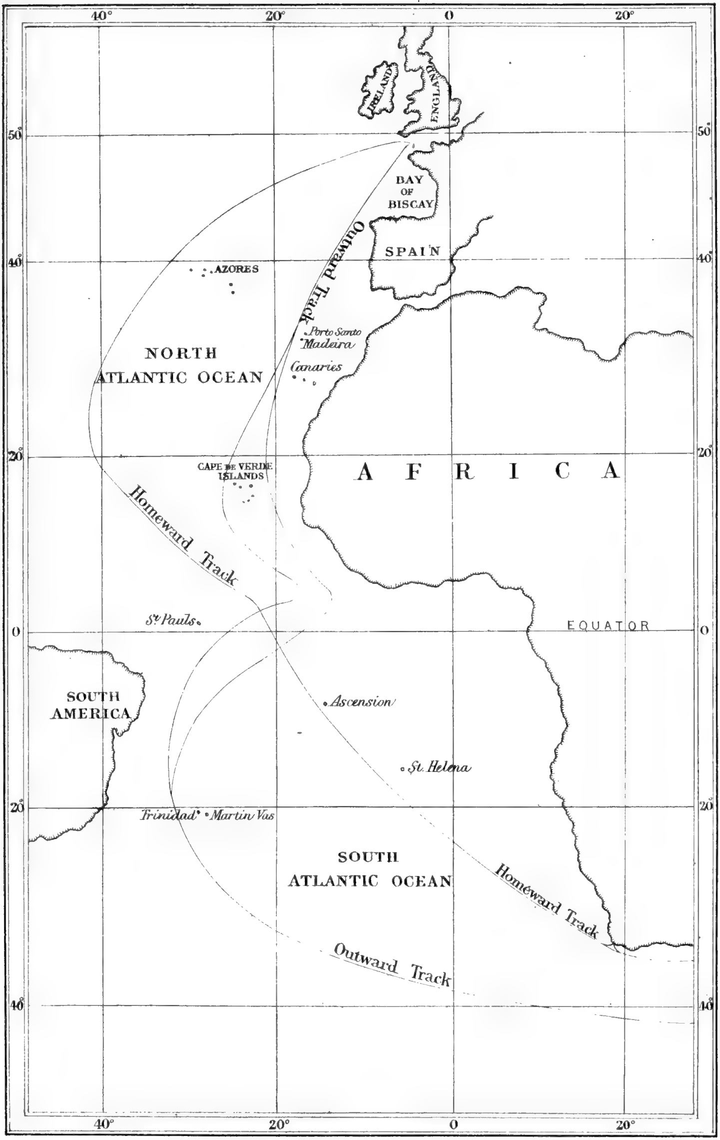
On the Passage of Chloride of Strontium into the Textures.

Two guinea-pigs, which had been given no strontium, had the whole kidney, liver, and lenses examined for strontium, but no trace of it could be found.

A guinea-pig was given four grains of chloride of strontium; in seven hours it was killed. The urine showed strontium distinctly in a single drop. No strontium could be detected in the kidney, liver, or lens, though a whole lens was taken for the examination by the spectrum analysis.

Another guinea-pig was given ten grains of chloride of strontium; in fourteen hours and a half it was killed. A small quantity of urine showed no strontium, and no strontium was found in the ashes of the kidney or liver.

To a third guinea-pig half a grain of chloride of strontium was given. Nineteen hours afterwards the urine showed traces of strontium, and then half a grain more was given. Twenty-four hours and a half afterwards another grain was given; and twenty-four hours after this half a grain more. Twenty-seven hours afterwards another half grain of chloride of strontium was given. At this time then the urine showed strontium very distinctly. On the sixth day another half grain, and again on the seventh, eighth, ninth, tenth, and eleventh day, until five grains and a half were taken. The twelfth day it was killed. The urine showed strontium distinctly. No strontium could be detected in the lens, humours, or blood; and minute traces only in the ash of the kidneys and liver.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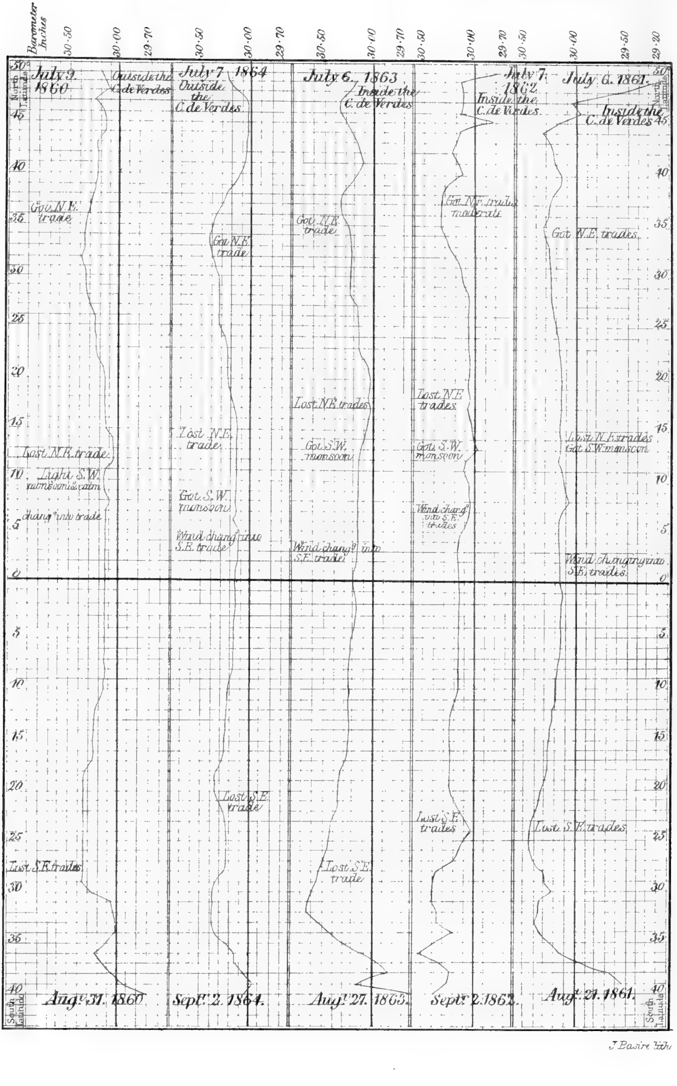
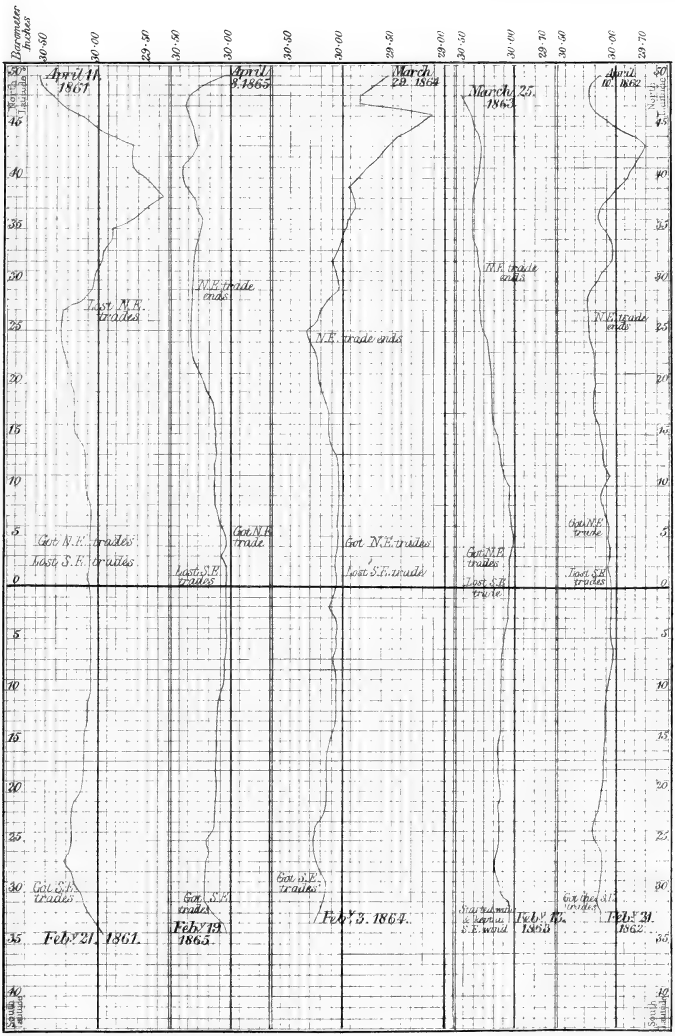


Passages outward-bound from England.



Passages homeward bound from the Cape.

J. Basire lith.

"An Account of the Base Observations made at the Kew Observatory with the Pendulums to be used in the Indian Trigonometrical Survey." By BALFOUR STEWART, M.A., LL.D., F.R.S., Superintendent of the Kew Observatory, and BENJAMIN LOEWY, Esq. Received June 13, read June 15, 1865.

Her Majesty's Indian Government, on the recommendation of the Royal Society, lately decided that pendulum observations shall be made at different stations in India in connexion with the Great Trigonometrical Survey of that country.

The object of these proposed observations may be stated in a very few words. The labours of those engaged in the Trigonometrical Survey have already disclosed the fact that the direction of the plumb-line in the northern stations of India was influenced to some extent by the mass of the Himalayas, and it was therefore thought highly desirable that the influence of these mountains upon the *intensity* of terrestrial gravity should be investigated in addition to their influence upon its *direction*. The propriety of this view will at once be evident, if we reflect that by knowing the change produced not only upon the direction of gravity, but also on its intensity, we know at once all the particulars of the disturbing mountain-force both as regards magnitude and direction.

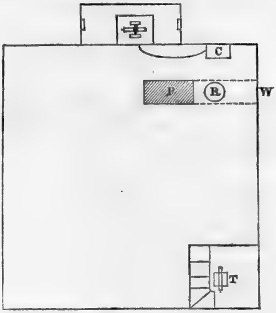
It was therefore with the view of ascertaining the alteration which these mountains might cause upon the intensity of gravity that the Indian pendulum observations were decided upon. In consequence of this decision, Captain Basevi, R.E., and first assistant in the Survey department, was appointed to superintend the observations, and instructed to repair to the Kew Observatory previously to his departure for India, in order to become acquainted with the necessary instruments, their adjustment, and the method of observing with them.

After attending daily at the Observatory from the beginning of September to the middle of November, this officer was perfectly instructed in every particular necessary for the practical part of these observations, as well as for their calculation and reduction. He was then obliged to leave for India, being prevented by his early departure from making the necessary base determinations with the instruments at Kew Observatory.

The best arrangement of apparatus formed the subject of careful discussion with Colonel Walker, Superintendent of the Indian Survey; and the experimental arrangements ultimately adopted received the sanction of this officer, who, besides suggesting several improvements, made himself thoroughly acquainted with all the details of the apparatus.

A room suitable for these observations was constructed in the south-east corner of the Observatory, the expenses of which were defrayed from the Government Grant Fund of the Royal Society.

The following simple diagram will be sufficient to show the experimental arrangement in the Pendulum-room.



C is the place for the clock, which is connected with the transit-instrument; P is the pillar bearing a slab attached also to the wall at W, to which the receiver (R) is rigidly fixed.

T is the telescope for the observations of the coincidences, mounted on a pillar which stands in a depression, so that the observer is not under the necessity of kneeling down during the observation.

In every other part the arrangement is entirely similar to that described by General Sabine in the Philosophical Transactions for 1829,—with this difference, that the receiver was in our experiments a copper one with glass windows. The whole of the apparatus was made by Mr. P. Adie, who deserves the highest praise for the excellent manner in which the work was executed by him.

The pendulums used were those marked No. 1821 and No. 4, used formerly by General Sabine in different parts of the globe. The former was also used by Mr. Airy in his Harton experiments.

Method of registering and reducing the Observations.

The manner in which the number of vibrations, made by a detached pendulum, are determined from a series of observed coincidences with the pendulum of a clock has been so often described, that we may refer to the writings of Kater, Sabine, Baily, and others on the subject. The established methods have been followed throughout in these experiments, and

the only change introduced was a very slight one, with the view of obtaining a more correct arc of vibration.

It is usual to observe the arc a little after the coincidence, which does not give the true arc corresponding to it. To obviate this, the arc was read in our series about 30 seconds before, and again 30 seconds after each observed coincidence, marking first the right edge of the tailpiece and then the left one. If we call these four readings of the scale a, b, c, d, we may consider

$$\frac{(a \sim b) + (c \sim d)}{2}$$

as a very exact representation of the reading for the *arc* at the instant of the coincidence.

The adjustment of the diaphragm, disk, and tailpiece was made very carefully at the commencement of the experiments. Nevertheless it was found slightly deranged when the pendulum was reversed. In this case, as is well known, the disappearance and reappearance of the disk are not each instantaneous; but we see first one side of the disk, then the other disappear, and in the same order reappear, so that we have four events, of which, calling the time of their happening respectively α, β, γ, δ, either

$$(1)\ \frac{\alpha+\beta+\gamma+\delta}{4}, \quad \text{or } (2)\ \frac{\alpha+\delta}{2}, \quad \text{or } (3), \text{ lastly, } \frac{\beta+\gamma}{2}$$

will give us the *time* of coincidence. In a few sets of our series the first formula was used; but it was soon found that the correct registration in such a case is a matter of the greatest difficulty, and it was therefore thought in one instance preferable to stop the clock and repeat the adjustment, and afterwards a similar derangement was rectified by a lateral motion of the observing telescope. With a few trials, using a few successive coincidences for the purpose, it is quite possible to adjust the whole to the greatest nicety without stopping the clock.

The reduction of the observations was made entirely after the manner of former experimenters. It comprises the following corrections:—

A. *Correction of the observed arc-readings and reduction of the vibrations to infinitely small arcs.* In the first place, the scale for reading the arc being behind the tailpiece of the pendulum, the registered readings are too large.

Let D be the distance of the scale from the object glass of the telescope, d its distance from the tail of the pendulum, O the observed reading of the *whole* arc on the scale graduated from end to end, S the distance of the indicating-point of the tailpiece from the knife-edge, then the true arc, or more correctly *semiarc* observed $(=\alpha)$, through which the pendulum moved from the vertical, is given by the formula

$$\operatorname{tang} \alpha = \frac{\mathrm{O}(\mathrm{D}-d)}{2\mathrm{DS}},$$

expressing all distances in inches, into which the scale was divided.

The values of $\frac{D-d}{2DS}$ were determined for each pendulum from accurate measurements, and are

For pendulum No. 4 in position, face on $=\frac{98 \cdot 74}{2 \times 100 \cdot 22 \times 49 \cdot 89}$

" " " " face off $=\frac{99 \cdot 67}{2 \times 100 \cdot 22 \times 49 \cdot 89}$

" " No. 1821 " face on $=\frac{99 \cdot 47}{2 \times 100 \cdot 22 \times 49 \cdot 3}$

" " " " face off $=\frac{98 \cdot 95}{2 \times 100 \cdot 22 \times 49 \cdot 3}$.

The logarithms of these expressions were added to those of the observed readings for the logarithm of the tangent of α.

In the next place, the reduction to infinitely small arcs was deduced from the well-known formula,

$$\text{Number of infinitely small vibrations} = n + n \times \frac{M \sin(\alpha+\alpha') \sin(\alpha-\alpha')}{32(\log \sin \alpha - \log \sin \alpha')} *,$$

where M denotes the logarithmic modulus $=0 \cdot 4342945$; α the initial, and α' the final semiarc of vibration, expressed in degrees, minutes, and seconds, n being the number of observed vibrations; and to obtain a more correct result from this formula, the calculation was made *for each interval* between two successive observations.

B. The *rate of the clock* was determined from a series of observations of star-transits, the results of which are given in Table I. The rate was somewhat unequal during the experiments, the range being equal to $\frac{6}{10}$ths of a second; and besides, the unfavourable state of the weather occasioned longer intervals between the observations than was desirable. To free the results as far as possible from any errors arising from this source, the rates were represented in a series, as shown in Table II., which also gives the actual number of vibrations made by the sidereal clock in a mean solar day, as deduced from the following formula:—

$$\text{Number of vibrations in a mean solar day} = N' = 86636 \cdot 5554\left(1 - \frac{r}{86400}\right),$$

where r is the observed rate, which in our case was a losing one throughout the whole of the observations.

If we now call V′ the number of observed vibrations of the clock-pendulum from beginning to the end of one experiment, V the number of observed vibrations of the detached pendulum during the same time, corrected for the amplitude of the arc, and finally N′ the number of actual vibrations of the clock in a mean solar day at the date of the experiment, found as above, we have for the number of infinitely small vibrations of

* See Memoirs of the Royal Astronomical Society, vol. vii. p. 22.

the detached pendulum during a mean solar day the following proportion:

$$V' : V :: N' : N,$$

$$N = \frac{VN'}{V'}.$$

Table I.—List of Transits observed in connexion with the Pendulum Experiments for India, and clock-rates deduced from them.

Date.	Name of Star.	Right Ascension.	Time of passing mean wire.	Sum of instrumental errors.	Error of clock.	Mean rate deduced.
1865.		h m s	h m s	s	m s	
January 7.	β Arietis	1 47 12·40	1 47 7·96	+0·76	− 3·68	
	α Arietis	1 59 35·42	1 59 30·96	+0·70	− 3·76	
	ξ^2 Ceti	2 20 60·28	2 20 55·42	+1·03	− 3·83	
	α Persei	3 14 44·47	3 14 40·84	−0·17	− 3·80	
	η Tauri	3 39 29·68	3 39 25·16	+0·68	− 3·84	
	γ′ Eridani	3 51 45·34	3 51 40·06	+1·48	− 3·82	
	α Tauri	4 28 12·58	4 29 39·4	+0·85	− 3·85	
„ 8.	η Bootis	13 48 15·68	13 48 9·98	+1·06	− 4·64	
	α Bootis	14 9 30·38	14 9 24·72	+1·04	− 4·62	
	ρ Bootis	14 26 0·57	14 25 55·16	+0·81	− 4·60	s
	ε Bootis	14 39 5·26	14 38 59·68	+0·89	− 4·69	−1·81
„ 9.	η Bootis	13 48 15·71	13 48 8·4	+0·84	− 6·47	
	α Bootis	14 9 30·42	14 9 22·98	+0·82	− 6·62	
	ρ Bootis	14 26 0·61	14 24 53·74	+0·55	− 6·32	
	ε Bootis	14 39 5·30	14 38 58·2	+0·64	− 6·46	−1·83
„ 13.	α Bootis	14 9 30·54	14 9 16·1	+0·97	− 13·47	
	ε Bootis	14 39 5·43	14 38 51·02	+0·81	− 13·60	−1·76
„ 14.	ε Bootis	14 39 5·46	14 37 4·56	+0·88	−2 0·02	
	α^2 Libræ	14 43 25·37	14 41 23·70	+1·65	−2 0·02	
	υ Piscium	1 34 25·30	1 32 23·44	+1·12	−2 0·74	
	β Arietis	1 47 12·31	1 45 10·66	+0·79	−2 0·86	
	α Arietis	1 59 35·33	1 57 33·8	+0·73	−2 0·80	
	ξ^2 Ceti	2 21 0·20	2 18 58·28	+1·06	−2 0·86	
	γ Tauri	3 39 29·61	3 37 27·98	+0·71	−2 0·92	−1·64
„ 16.	ρ Bootis	14 26 0·85	14 23 56·72	+0·79	−2 3·34	
	ε Bootis	14 39 5·52	14 37 1·16	+0·87	−2 3·49	
	α^2 Libræ	14 43 25·43	14 41 20·18	+1·65	−2 3·60	−1·76
„ 17.	α Bootis	14 9 30·67	14 9 45·86	+1·13	+ 16·32	
	ρ Bootis	14 26 0·89	14 26 16·3	+0·92	+ 16·33	
	ε Bootis	14 39 5·55	14 39 21·0	+0·98	+ 16·43	
	α^2 Libræ	14 43 25·46	14 43 40·22	+1·69	+ 16·45	
„ 19.	η Tauri	3 39 29·55	3 39 41·06	+0·57	+ 12·08	
	ε Tauri	4 20 46·05	4 20 57·54	+0·69	+ 12·18	
	α Tauri	4 28 12·49	4 28 23·78	+0·76	+ 12·05	−1·67
„ 20.	α Persei	3 14 44·22	3 14 54·92	−0·33	+ 10·39	
	γ′ Eridani	3 61 45·20	3 51 54·16	+1·41	+ 10·37	
	ε Tauri	4 20 46·04	4 20 55·6	+0·81	+ 10·37	
	α Tauri	4 28 12·48	4 28 21·94	+0·83	+ 10·29	
	α Aurigæ	5 6 46·01	5 6 56·2	−0·03	+ 10·16	−1·77
„ 22.	α Persei	3 14 44·18	3 14 51·28	−0·17	+ 6·93	
	η Tauri	3 39 29·52	3 39 35·74	+0·67	+ 6·89	
	γ′ Eridani	3 51 45·18	3 51 50·66	+1·46	+ 6·94	
	ε Tauri	4 20 46·02	4 20 52·16	+0·88	+ 7·02	
	α Tauri	4 28 12·46	4 28 18·38	+0·94	+ 6·86	−1·71

TABLE I. (*continued.*)

Date.	Name of Star.	Right Ascension.	Time of passing mean wire.	Sum of instrumental errors.	Error of clock.	Mean rate deduced.
1865.		h m s	h m s	s	m s	
January 28.	β Tauri	5 17 47·76	5 17 46·36	+0·96	− 0·44	
	δ Orionis	5 25 8·50	5 25 6·5	+1·42	− 0·58	
	ε Orionis	5 29 23·66	5 29 21·72	+1·44	− 0·50	s
	α Orionis	5 47 53·80	5 47 51·96	+1·31	− 0·53	−1·22
Febr. 9.	γ′ Eridani	3 51 44·90	3 51 29·06	+1·53	−14·31	
	ε Tauri	4 20 45·78	4 20 30·58	+0·81	−14·39	
	α Tauri	4 28 12·23	4 27 56·98	+0·87	−14·38	
	ι Aurigæ	4 48 14·34	4 47 59·3	+0·63	−14·41	
	ε Leporis	4 59 46·14	4 59 30·06	+1·79	−14·29	
	α Aurigæ	5 6 45·68	5 6 31·16	+0·22	−14·30	
	β Tauri	5 17 47·61	5 17 32·26	+0·88	−14·47	
	α Leporis	5 26 48·22	5 26 31·96	+1·73	−14·53	−1·16
„ 17.	ι Aurigæ	4 48 14·29	4 47 48·36	+0·41	−25·52	
	ε Leporis	4 59 45·99	4 59 18·68	+1·73	−25·58	
	α Aurigæ	5 6 45·52	5 6 19·98	−0·06	−25·60	
	β Tauri	5 17 47·47	5 17 21·36	+0·54	−25·57	
	δ Orionis	5 25 8·24	5 24 41·4	+1·22	−25·62	−1·40
„ 19.	μ Geminorum..	6 14 49·62	6 14 20·16	+0·50	−28·98	
	γ Geminorum..	6 29 56·79	6 29 27·08	+0·66	−29·05	
	α Canis Majoris	6 39 13·88	6 38 43·26	+1·50	−29·12	−1·69
„ 20.	ι Aurigæ	4 48 14·13	4 47 42·58	+0·82	−30·73	
	ε Leporis	4 59 45·93	4 59 13·4	+1·84	−30·69	
	α Aurigæ	5 6 45·43	5 6 14·26	+0·45	−30·72	
	β Tauri	5 17 47·42	5 17 15·7	+0·92	−20·80	
	α Leporis	5 26 48·03	5 26 15·48	+1·75	−30·80	−1·80

TABLE II.—Showing the rate of the clock, and the number of its vibrations during a mean solar day.

No. f exp.	Rate (sid. time).	No. of vibr. in a mean solar day =N′.	No. of exp.	Rate (sid. time).	No. of vibr. in a mean solar day =N′.	No. of exp.	Rate (sid. time).	No. of vibr. in a mean solar day =N′.
	s			s			s	
1	−1·76	86634·795	21	−1·16	86635·395	41	−1·40	86635·155
2	−1·76	86634·795	22	−1·16	86635·395	42	−1·40	86635·155
3	−1·76	86634·795	23	−1·16	86635·395	43	−1·46	86635·095
4	−1·76	86634·795	24	−1·16	86635·395	44	−1·52	86635·035
5	−1·64	86634·915	25	−1·16	86635·395	45	−1·52	86635·035
6	−1·64	86634·915	26	−1·16	86635·395	46	−1·58	86634·975
7	−1·64	86634·915	27	−1·16	86635·395	47	−1·58	86634·975
8	−1·76	86634·795	28	−1·16	86635·395	48	−1·64	86634·915
9	−1·76	86634·795	29	−1·16	86635·395	49	−1·64	86634·915
10	−1·67	86634·885	30	−1·16	86635·395	50	−1·64	86634·915
11	−1·67	86634·885	31	−1·16	86635·395	51	−1·64	86634·915
12	−1·77	86634·785	32	−1·16	86635·395	52	−1·69	86634·865
13	−1·73	86634·825	33	−1·16	86635·395	53	−1·69	86634·865
14	−1·69	86634·865	34	−1·16	86635·395	54	−1·69	86634·865
15	−1·26	86635·295	35	−1·22	86635·335	55	−1·69	86634·865
16	−1·24	86635·315	36	−1·22	86635·335	56	−1·69	86634·865
17	−1·22	86635·335	37	−1·28	86635·275	57	−1·75	86634·805
18	−1·20	86635·355	38	−1·28	86635·275	58	−1·75	86634·805
19	−1·18	86635·375	39	−1·28	86635·275	59	−1·75	86634·805
20	−1·18	86635·375	40	−1·34	86635·215	60	−1·75	86634·805

C. *Correction for temperature.*—Two thermometers were fixed, one to the lower, the other to the upper part of a brass bar, which was made by Mr. Adie, of precisely the same form as the pendulums.

The brass bar being fixed near the middle of the receiver, close to the swinging pendulum, every change in the temperature of the latter was of course shared by the brass bar, and indicated by the two thermometers, which were extremely sensitive and read to ·05 of a degree. The readings of these two thermometers were in the first instance corrected for index-error. The instruments having been very carefully compared with the Kew Standard, a table of index-errors was made from these comparisons, and, by interpolation, giving the errors from degree to degree. Another correction was applied for the observations in the exhausted receiver on account of the effect of exhaustion on the glass tubes of the thermometers. This effect was determined very accurately by a series of experiments at Kew, and found to be equal for both thermometers, and amounting to 0°·43 for a decrease in pressure of 29·210 inches. This correction is smaller than that assumed by General Sabine and the late Mr. Baily, who make it ¾ of a degree for the thermometers which they employed.

Our experiments showed the remarkable fact that the correction is by no means proportional to the decrease in pressure. The diminution of the pressure from 30·080 inches to 13·610, that is, by an amount of 16·470 inches, gave a correction of only 0°·052, while a further decrease of 12·820, bringing the pressure to 0·790 inch, gave for one thermometer 0°·377, and for the other 0°·385.

The mean of the upper and lower thermometer reading will give the temperature of the pendulum at the moment of the observations; and if we call t, t', t'', t''' the temperatures found in this manner for the successive observations, we have

$$\frac{t+t'}{2},\ \frac{t'+t''}{2},\ \frac{t''+t'''}{2}$$

as the most probable temperature during the interval between two consecutive observations. These intervals being of unequal length, we will call n, n', n'', n''', the number of coincidence-intervals which they contain; and calling t° the mean temperature of the whole experiment, we have

$$^\circ = \frac{n\left(\frac{t+t'}{2}\right)+n'\left(\frac{t'+t''}{2}\right)+n''\left(\frac{t''+t'''}{2}\right)\ldots\ldots}{n+n'+n''\ldots\ldots}$$

Table III. gives the mean temperature found in this manner for each experiment, and shows the mean of all observed temperatures for each pendulum, to which temperature all the experiments made with that pendulum have been reduced. For this reduction it would have been best if we had had an opportunity of swinging the pendulums at extremes of temperature, say about 50° distant from each other. But the desirability of sending the apparatus to India under the care of Mr. Hennessey, who left by the March mail,

prevented such a course, and we availed ourselves of the elaborate series of experiments on the temperature corrections of pendulums, made by General Sabine (*vide* Phil. Trans. 1830, p. 251), which gives 0·44 vibration per diem for each degree of Fahrenheit's scale. General Sabine found in a former series this correction nearer to 0·43; and he says, in the above mentioned publication, "The probable error which may be incurred by employing the correction 0·44 for each degree as now determined, can only be very inconsiderable; but when the differences of temperature amount to 50°, which is a case of actual experience in pendulum observations, the question of whether 0·43 or 0·44, for example, be the more correct value, involves an uncertainty in the ultimate result of no less than half a vibration a day."

The pendulums which we used were not those employed by General Sabine in his determinations, but they were made by the same maker at the same time, and very probably from the same kind of brass, and there cannot be the least doubt that the true correction will lie between 0·43 and 0·44. We have therefore adopted 0·435 for our reductions; and as the greatest difference in temperature between a single experiment and the mean is less than 11°, the greatest error would in this case amount only to $\frac{5}{100}$ths of a vibration per diem, an error too small to affect seriously the mean result of the whole.

At the same time we must state that, as Colonel Walker and Captain Basevi inform us, experiments will be made in India with both pendulums, to ascertain their exact constants with regard to expansion, and that our results will of course have then to be modified accordingly.

TABLE III.—Showing the Mean Temperature for each experiment, and the Mean of the whole series for each Pendulum.

Pendulum No. 1821.		Pendulum No. 4.	
No. of experiment.	Mean temperature.	No. of experiment.	Mean temperature.
	°		°
1	57·963	1	55·869
2	54·573	2	52·692
3	53·750	3	52·116
4	54·460	4	53·586
5	52·890	5	54·269
6	52·300	6	51·047
7	52·233	7	51·432
8	53·339	8	52·093
9	53·397	9	53·972
10	49·154	10	50·794
11	49·075	11	45·631
12	51·649	12	50·120
13	49·789	13	50·455
14	50·795	14	57·081
15	47·723	15	56·737
16	48·813	16	58·894
17	48·398	17	59·412
18	48·843	18	60·954
19	48·203	19	60·597
20	46·264	20	62·560
21	46·515	21	65·551
22	50·892	22	59·162
23	50·077	23	61·489
24	50·352	24	62·183
25	54·051	25	64·151
26	55·714	26	66·690
27	55·536		
28	56·561		
29	58·344		
Mean	51·781	Mean	56·520

D. *Correction for pressure of air.*—This correction, as shown in the Phil. Trans. for 1832, is thus determined :—

Let β' denote the reading of the gauge for the mean of the experiments made in air, and β'' the same reading for the mean of the vacuum experiments; also let t° denote the mean temperature of all the experiments, both in air and vacuo, then the expression

$$\frac{\beta'-\beta''}{1+\cdot 0023(t^\circ-32)}$$

will denote very nearly the mean difference of density between the two sets of experiments.

Now if N′ denote the mean number of vibrations *in air* during a mean solar day, and N″ the mean number of vibrations *in vacuo* during the same time, then the constant for one inch of reduced pressure will be

$$C'=\frac{N''-N'}{\beta'-\beta''}\left(1+\cdot 0023(t^\circ-32)\right).$$

Hence if β denote the actual mean pressure for a single experiment and

t the mean temperature of that particular experiment, the final correction for that experiment will then be found

$$C=C'\times\frac{\beta}{1+\cdot0023(t-32)}.$$

The following Table (IV.) gives the elements for obtaining the constant C′ for both pendulums.

TABLE IV.—Elements for deducing the Constant C′ from the Experiments in Air and in the Exhausted Receiver.

Pendulum No. 1821, Position "Face on."							
In Air.				In the Exhausted Receiver.			
No. of experiment.	Number of corrected vibrations.	Pressure.	Temperature.	No. of experiment.	Number of corrected vibrations.	Pressure.	Temperature.
		in.	°			in.	°
I.	86063·840	29·793	57·96	I.	86072·053	2·165	49·15
II.	86063·881	29·381	54·57	II.	86072·725	1·325	49·07
III.	86063·818	29·193	53·75	III.	86072·643	1·402	51·65
IV.	86064·126	29·153	54·46	IV.	86072·917	0·943	49·79
V.	86063·771	29·048	54·05	V.	86072·846	1·944	50·80
VI.	86064·240	29·103	55·71	VI.	86072·495	0·905	47·72
VII.	86064·202	29·222	55·54				
VIII.	86064·138	29·271	56·56				
IX.	86064·015	29·362	58·34				
Means...	86064·015	29·281	55·66	Means...	86072·613	1·447	49·70
Pendulum No. 1821, Position "Face off."							
		in.	°			in.	°
I.	86064·362	28·906	52·89	I.	86073·049	0·781	48·81
II.	86064·206	28·718	52·30	II.	86072·769	1·461	48·40
III.	86064·354	28·990	52·23	III.	86072·349	1·031	48·84
IV.	86064·578	29·180	53·34	IV.	86072·385	0·814	48·20
V.	86064·423	29·068	53·40	V.	86072·200	0·960	46·26
VI.	86063·433	29·378	50·89	VI.	86072·289	1·513	46·51
VII.	86063·375	29·279	50·08				
VIII.	86062·738	29·005	50·35				
Means...	86063·934	29·065	51·94	Means...	86072·507	1·093	47·84
Pendulum No. 4, Position "Face on."							
		in.	°			in.	°
I.	86162·815	29·926	55·87	I.	86171·544	1·649	57·08
II.	86162·495	30·159	52·69	II.	86171·213	1·831	56·74
III.	86162·518	30·427	52·12	III.	86170·853	2·865	58·89
IV.	86162·351	30·459	53·59	IV.	86170·518	3·340	59·41
V.	86162·394	30·532	54·27	V.	86170·909	3·839	60·95
VI.	86162·774	29·718	60·60				
VII.	86163·228	29·709	62·56				
VIII.	86163·337	29·683	65·55				
Means...	86162·739	30·077	57·16	Means...	86171·007	2·705	58·61

TABLE IV. (*continued.*)

Pendulum No. 4, Position "Face off."							
In Air.				In the Exhausted Receiver.			
No. of experiment.	Number of corrected vibrations.	Pressure.	Temperature.	No. of experiment.	Number of corrected vibrations.	Pressure.	Temperature.
		in.	°			in.	°
I.	86162·460	30·631	51·05	I.	86172·510	1·491	50·79
II.	86162·187	30·638	51·43	II.	86171·026	1·973	47·75
III.	86162·994	30·640	52·09	III.	86172·383	0·575	45·63
IV.	86162·933	29·355	59·16	IV.	86172·236	0·637	50·12
V.	86163·613	29·303	61·49	V.	86171·482	0·535	50·45
VI.	86163·484	29·306	62·18				
VII.	86163·480	29·477	64·15				
VIII.	86163·757	29·647	66·69				
Means...	86163·113	29·874	58·53	Means...	86171·927	1·042	48·95

Determination of the Constant C′ from the above Mean Results.

	$N''-N'$.	$\beta'-\beta''$.	$1+\cdot0023(t^\circ-32)$.	Value of C′.
Pendulum No. 1821, Face on...	8·609	27·834	1·047564	0·324010
,, ,, ,, Face off...	8·573	27·972	1·041147	0·319096
Pendulum No. 4, Face on	8·268	27·372	1·059524	0·320040
,, ,, ,, Face off	8·814	28·832	1·050002	0·320988

E. The *reduction of the resulting number of vibrations to the sea-level* is calculated from

$$\frac{N}{R}\times h\times x,$$

where R is the earth's radius at the latitude of the Kew Observatory, h the height of the receiver above the mean level of the sea, and x a quantity which, with Dr. Young, may be assumed for a tract of level country to be $=\cdot666$ (*vide* Phil. Trans. for 1819, page 98).

This correction has been only applied to the ultimate mean number of vibrations of each pendulum.

Taking Bessel's value for the semiaxis major and the eccentricity of the earth, and $h=17\cdot5$ feet as given by measurement and the known height of our standard barometer, the logarithm of the factor for this correction is $\bar{7}\cdot7467623$.

Result.

Adopting the values for the reduction to a vacuum as found in Table IV., and applying the correction to those experiments, which were made in a highly rarefied medium, we find the following numbers of vibrations made by each pendulum in both positions in a *mean solar* day *in vacuo*, viz. for

Pendulum No. 1821, Face on, Exp.	I.	86072·728	Mean: 86073·064
	II.	86073·138	
	III.	86073·078	
	IV.	86073·211	
	V.	86073·450	
	VI.	86072·777	
,, ,, Face off, Exp.	I.	86073·289	Mean: 86072·844
	II.	86073·218	
	III.	86072·668	
	IV.	86072·635	
	V.	86072·497	
	VI.	86072·756	
Pendulum No. 4, Face on, Exp.	I.	86172·043	Mean: 86171·822
	II.	86171·767	
	III.	86171·717	
	IV.	86171·524	
	V.	86072·061	
,, ,, Face off, Exp.	I.	86172·969	Mean: 86172·249
	II.	86171·637	
	III.	86172·562	
	IV.	86172·432	
	V.	86171·647	

And reducing the means to the sea-level, we obtain the following

Final Result:—

Pendulum No. 1821,	Face on, 86073·112	vibrations in a mean solar day.
,, ,, ,,	Face off, 86072·892	,, ,, ,,
Pendulum No. 4,	Face on, 86171·870	,, ,, ,,
,, ,, ,,	Face off, 86172·297	,, ,, ,,

Finally, we give an example of one experiment, with the mode of its reduction.

EXAMPLE OF THE OBSERVATIONS AND REDUCTIONS.

Experiment No. 12. January 20th, 1865. Pendulum No. 1821, Face on. In the exhausted Receiver. The clock making 86634·785 vibrations in a mean solar day.

Observation.

Number of coincidence.	Time of first disappearance.	Second disappearance.	Time of first reappearance.	Second reappearance.	Time of coincidence.	Reading of scale before		$a \sim b$.	Reading of scale after		$c \sim d$.	Thermometers.		Pressure.
						a.	*b.*		*c.*	*d.*		Upper.	Lower.	
	h m s	s	h m s	s	h m s							°	°	in.
1	2 6 36·5	36·5	2 6 58·5	58·5	2 6 57·5	0·60	2·86	2·26	1·29	3·53	2·24	53·15	53·05	0·768
2	12 3·5	3·5	12 5·5	5·5	12 4·5	0·62	2·84	2·22	1·30	3·53	2·23	53·20	53·10	
3	17 11·5	11·5	17 13·5	13·5	17 12·5	0·62	2·84	2·22	1·32	3·52	2·20	53·30	53·20	0·775
17	3 28 45·5	45·5	3 28 47·5	47·5	3 28 46·5	0·74	2·72	1·98	1·44	3·40	1·96	53·20	52·95	0·878
18	33 52·5	52·5	33 54·5	54·5	33 53·5	0·74	2·72	1·98	1·44	3·40	1·96	53·20	52·95	
19	38 59·5	59·5	39 1·5	1·5	39 0·5	0·75	2·72	1·97	1·45	3·39	1·94	53·25	53·00	0·882
153	15 6 19·5	19·5	15 6 27·5	27·5	15 6 23·5	1·45	2·01	0·56	2·16	2·70	0·54	49·90	49·45	1·775
155	16 35·5	35·5	16 45·5	45·5	16 40·5	1·45	2·00	0·55	2·16	2·69	0·53	49·65	49·25	1·782
159	37 8·5	8·5	37 19·5	19·5	37 14·0	1·47	1·99	0·52	2·17	2·68	0·51	49·40	49·05	1·805
183	17 40 35·5	35·5	17 40 45·5	45·5	17 40 40·5	1·53	1·94	0·41	2·23	2·63	0·40	50·20	50·00	1·952
184	45 42·5	42·5	45 54·5	54·5	45 48·5	1·53	1·94	0·41	2·23	2·62	0·39	50·25	50·10	
185	17 50 50·5	50·5	17 51 2·5	2·5	17 50 56·5	1·53	1·93	0·40	2·24	2·62	0·38	50·35	50·20	1·970

Preliminary corrections.

Correction of the Thermometer-Readings.				Computation of semiarc of vibration $=\alpha$.					
Corrected for index error.			Corrected for effect of exhaustion.	Mean reading of scale $=$O.	Logarithm of O.	Logarithm of tan α.	True semiarc $=\alpha$.		
Upper.	Lower.	Mean.							
°	°	°	°				°	′	″
53·17	52·755	52·962	53·392	2·250	0·3521825	8·3550433	1	17	51
53·22	52·805	53·012	53·442	2·225	0·3473300	8·3501908	1	16	59
53·32	52·905	53·112	53·542	2·210	0·3443923	8·3472531	1	16	28
53·22	52·655	52·937	53·367	1·970	0·2944662	8·2973270	1	8	10
53·22	52·655	52·937	53·367	1·970	0·2944662	8·2973270	1	8	10
53·27	52·705	52·987	53·417	1·955	0·2911468	8·2940076	1	7	39
49·92	49·21	49·565	49·995	0·550	9·7403627	7·7432235	0	19	2
49·67	49·01	49·340	49·770	0·540	9·7323938	7·7352546	0	18	41
49·43	48·81	49·120	49·550	0·515	9·7118072	7·7146680	0	17	49
50·22	49·74	49·980	50·410	0·505	9·6074550	7·6103158	0	14	1
50·27	49·84	50·055	50·485	0·400	9·6020600	7·6049208	0	13	51
50·37	49·94	50·155	50·585	0·390	9·5910646	7·5939254	0	13	30

Correction for amplitude of arc and rate.

Coincidence.	Vibrations of clock pendulum.	Vibrations of detached pendulum $=n$.	log. $\dfrac{\text{M}\sin(\alpha+\alpha')\sin(\alpha-\alpha')}{32\,(\log\sin\alpha-\log\sin\alpha')}=\log\text{A}$.	$\log n+\log\text{A}$.	Correction for amplitude, $+$.	Number of infinitely small vibrations during experiment.	Number of infinitely small vibrations per diem.
1 to 2	307·0	305·0	5·5009216	7·9852214	0·0096654		
2 — 3	308·0	306·0	5·4931423	7·9788637	0·0095250		
3 — 17	4294·0	4266·0	5·4412666	9·0712875	0·1178386		
17 — 18 } 18 — 19 }	614·0	610·0	5·3871234	8·1724532	0·0148749		
19 — 153	41243·0	40975·0	4·9438104	9·5563294	0·3600223	56271·5256659	86072·700
153 — 155	617·0	613·0	4·2743172	7·0617777	0·0011529		
155 — 159	1233·5	1225·5	4·2457725	7·3340858	0·0021582		
159 — 183	7406·5	7358·5	4·1249578	7·9917471	0·0098118		
183 — 184	308·0	306·0	4·0114300	6·4971514	0·0003142		
184 — 185	308·0	306·0	3·9951628	6·4808842	0·0003026		
	56639·0	56271·0			0·5256659		

Correction for temperature.

Coincidence.	Temperature of pendulum during interval $\frac{t+t'}{2}$.	Number of intervals.	$n\left(\frac{t+t'}{2}\right)$.	Mean temperature of the experiment.	Correction for temperature.	Number of vibrations per diem corrected for temperature.
1 to 2	53°·417	1	53·417	51°·649	−0·057 vibration.	86072·643
2 — 3	53·492	1	53·492			
3 — 17	54·454	14	762·356			
17 — 18	53·367	1	53·367			
18 — 19	53·392	1	53·392			
19 — 153	51·706	134	6928·604			
153 — 155	49·882	2	99·764			
155 — 159	49·660	4	198·640			
159 — 183	49·980	24	1199·520			
183 — 184	50·447	1	50·447			
184 — 185	50·535	1	50·535			
		184	9503·534			

Correction for pressure of air.

Mean pressure in the receiver during the experiment $= 1{\cdot}402$ inch $=\beta$.

Mean temperature in the receiver during the experiment $= 51^\circ{\cdot}65 = t$.

$t-32=19{\cdot}65$ log $=1{\cdot}2933626$ log $\beta=0{\cdot}1467480$

log ·0023 $=7{\cdot}3617278$ log C $=9{\cdot}5105577$

$\cdot0023\times(t-32)=0{\cdot}045195$ log $=8{\cdot}6550904$ $9{\cdot}6573057$

log $1+\cdot0023(t-32)=0{\cdot}0191973$.

Correction for pressure $=0{\cdot}435$ log $=9{\cdot}6381084$

Result of the Experiment.

Corrected vibrations: 86072·643

\+ 0·435 for pressure of air.

Number of vibrations *in vacuo*: 86073·078

"Some Observations on Birds, chiefly relating to their Temperature, with Supplementary additions on their Bones." By JOHN DAVY, M.D., F.R.S., &c. Received May 26, 1865 *.

The observations which I have now the honour to submit to the Royal Society, have been made with the hope of contributing something to the elucidation of the high temperature for which birds as a class are remarkable.

I. *Of the Temperature of the Common Fowl* (Gallus domesticus).

Mr. Hunter, in his paper entitled "Of the Heat, &c. of Animals and Vegetables," published in the Philosophical Transactions for 1778, states that he found the temperature of the common fowl, both male and female, in the intestinum rectum between 103° and 104° of Fahr. From such observations as I have made, both in Ceylon and in England, it would appear that the temperature of this bird is considerably higher. In the former I found it as high *in recto* as 110° and 111°, and this in December, when the average temperature of the atmosphere, in that part of the island where the trials were made, is about 77°, which was the temperature of the air at the very time. In the latter I have found it to vary from 107° to 109° *. That the temperature of the common fowl should be a little lower in England than in Ceylon, is no more than might be expected, from the analogy of the difference of temperature of man in the two climates; and, in accordance, in the fowl I have found that even in England there is a slight difference in favour of the warmest months, comparing the results then obtained with those in the coldest.

Of the want of agreement between Mr. Hunter's results and mine I can offer no satisfactory explanation. I have thought it right to advert to them, he being so deservedly a high authority in physiology. Were his results to be depended on, then, were the common fowl to be considered as a fair example of the temperature of birds generally, they could hardly be considered as a class peculiar for highness of temperature, some of the mammalia having a temperature differing but little from that which he assigns to the common fowl †. Or, if not a fair example, then an exception, and the

* Read June 15, 1865. See Abstract, page 337.

† The trials from which the last-mentioned results were obtained have been made during the last two or three years, using a delicate thermometer of Negretti and Zambra made for the purpose, which had been compared with a standard instrument. The fowls tried were all barn-door fowls, living at large, and having the run of a field. The number of females examined was 37, of males 25. The mean temperature of the former *in recto* was 107°·64; the highest 109°, the lowest 107°; of the latter the mean temperature was 108°·25, the highest 109°, the lowest 107°.

‡ In Ceylon I found the temperature of the blood of the wild hog, as it flowed from the divided great cervical vessels, 106°, and that of the pig in England, in two instances, of the same degree; both were in excellent condition, and were killed in December; of one the temperature of the blood was tried; of the other, the cavity of the abdomen. The temperature of the sheep I have found to vary from 103° to 105° *in recto*; the latter in Ceylon.

common fowl would have to be placed amongst those birds, few in number, chiefly palmipedes, ocean-birds, peculiar for lowness of temperature*. Now, as neither of these conclusions is admissible, it seems unavoidable that Mr. Hunter's results must be received as inaccurate.

II. *Of the expired Air, and of the Air in the Air-receptacles and Bones of Birds.*

1. *Of the expired air.*—That which I have examined has been obtained from birds in the act of drowning. It is worthy of remark, I may premise, and I am not aware that the fact has been noticed by any previous inquirer, that different birds vary as to their power of retention of life under water. The goose expires I have found in about ten minutes; the duck in about the same time; the common barn-door fowl in about four or four and a half minutes; the turkey in about three minutes; the jay in about a minute and a half; the pigeon, the carrion-crow, rook, jackdaw, in about a minute; the robin, the hedge-warbler in about the same time; the blackbird in about three-quarters of a minute; the tawny owl, the bullfinch, the house-sparrow, in about half-a-minute. Those birds which are capable of retaining the air longest emit little air commonly when first submerged; but later, shortly before the extinction of life, they expel it in large quantities; those, on the contrary, especially the smaller birds, which soonest die, expel no air in the act of drowning.

I have examined the air from the goose in one instance only; it was a portion of the last emitted. Tested by milk of lime and phosphorus, it was found to consist of 7·5 carbonic acid gas, 92·5 azote.

The air from a duck, a small portion collected after four minutes' submersion, was composed of 2·38 carbonic acid, 9·52 oxygen, 88·10 azote. From another duck two portions of air were tried, one after five minutes' submersion, the other after between eight and ten. The first consisted of 7·5 carbonic acid, 7·5 oxygen, 85 azote; the second of 15·7 carbonic acid, 4·1 oxygen, 80·2 azote.

From the common fowl the air was examined in two instances; in both it was that which was emitted near death. Of one, the composition was 6·18 carbonic acid, 5·08 oxygen, 82·84 azote; of the other, 3·3 carbonic acid, 7·79 oxygen, 88·89 azote.

From a pigeon, the air emitted (it was pretty considerable in quantity) consisted of 11·1 oxygen, 89·7 azote.

From these results, and from a few others which I have obtained, it would appear that in the air expired by birds in the act of drowning there

* The temperature of the *Procellaria æquinoctialis* in one instance I found 103°·5 *in recto*, in another 105°. Dr. Brown-Séquard has made similar observations. See his 'Journal de la Physiologie' for January 1858. M. Ch. Martins has found the temperature of some sea-birds even lower, that of *Procellaria glacialis* 102°, of *Larus ridibundus* 104°. See his very interesting memoir on the Temperature of Northern Birds in the same Journal, and in the Number before quoted.

is a certain loss of carbonic acid, a loss equivalent to the proportion of oxygen less than exists in the atmospheric air inspired; and it may be inferred that the deficient carbonic acid was absorbed and retained in the blood; and that it was so, was indicated by the very dark colour of the blood obtained by the division of the great cervical vessels immediately after the extinction of life, and further by the large quantity of air that was disengaged from the blood when subjected to the air-pump *.

2. *Of the air from the air-sacs.*—On the air from these receptacles I have made the following experiments :—

From a turkey killed by drowning, a portion of air was collected by a puncture made under water into the air-vesicles under the sternum. It was found to consist of 15·5 carbonic acid, 84·5 azote.

From a duck deprived of life in the same manner, a portion of air was obtained from the abdominal air-receptacles. It was composed of 10·52 carbonic acid, 5·26 oxygen, 88·32 azote.

These results would seem to warrant the inference that the very delicate membrane of which the air-receptacles are formed, is like that of the air-cells of the lungs pervious to air; and the further inference, that the deficient carbonic acid in the air examined was owing to its absorption by the blood.

3. *Of the air contained in the bones.*—The experiments I have made on this air have been confined chiefly to that of the humerus. I may premise that in every instance in which I have examined the lining membrane of the hollow bones of birds (the air-containing bones), I have found it distinctly vascular; in this respect differing from the membrane of the air-receptacles communicating with the lungs situated in its thoracic and abdominal cavities. Not unfrequently, both in the humeri and femora, the vessels have had the appearance of being varicose, and this when the examination was made a few minutes after death.

From the humerus of a common fowl, killed by drowning, a portion of air obtained was found to be composed of 4·7 oxygen, 95·3 azote. The bone was dissected out under water, and its head there removed to allow free exit to the included air.

From the humerus of another fowl killed by the division of the great cervical vessels, the air procured consisted of 8·3 carbonic acid, 8·3 oxygen, 83·4 azote. In this instance the bone was dissected out under water, whilst the fowl was still warm. No air escaped until the delicate bony tissue (the reticulated structure) was broken through, and indeed then but little, until the head of the bone had been removed.

* Two trials of the blood were made, one in which the blood was received in water previously purged of air, the other in which it was received in weak solution of potassa, also exhausted of air by the pump: the difference was remarkable, so much air being disengaged from the first, so little from the second.

It may be deserving of mention that in the instance in which the blood diluted with water was allowed to coagulate, no air was disengaged by the action of the pump until the resisting clot was broken up, when the disengagement on exhaustion was copious.

From a third fowl, a cock weighing ten pounds and three-quarters, killed in the same manner as the last, the air from the humerus, measuring one-tenth of a cubic inch, consisted of 15 oxygen and 85 azote.

From the humerus of a rook, a few minutes after the bird had been shot, the air obtained, measuring ·22 cubic inch, was composed of 11 carbonic acid, 89 azote.

From the humerus of a tawny owl, three days after the death of the bird by drowning, the air collected consisted of 5·5 carbonic acid, 5·5 oxygen, 89 azote.

Though these results are not so uniform as might be expected, they seem to prove that the air in the bones undergoes the same change as in the air-sacs, and that there is an absorption, more or less, of the carbonic acid formed by the blood contained in the vessels of the lining membrane, the quantity varying according to circumstances. It may be conjectured that the difference in the results may partly be owing to the air-passage, the foramen or foramina, in the head of the bone, being more free in some instances than in others

III. *On Pulmonary and Cutaneous Aqueous Exhalation.*

The loss of water by exhalation from the lungs in the air expired, and from the cutaneous covering of the body by evaporation, must be considered material elements in the problem of the animal heat of birds. And inasmuch as birds drink but little, inasmuch as their skin generally is very thin, dry, and little vascular; further, as the air in expiration has to pass over a considerable length of surface of comparatively low temperature before it enters the open air, their loss of heat owing to these conditions must be small, and more especially so, taking into account the admirable covering of feathers, such bad conductors of heat, with which they are provided.

The only experiments I have to describe bearing in part on what has just been stated, chiefly the last-mentioned, are the following on the rate of cooling.

Two fowls, hens of the same brood, were selected for trial. The weight of each after loss of blood, having been killed by the division of the great cervical vessels, was five pounds. The temperature of one (No. 1), ascertained just before, was 107°·25 *in recto*; of the other (No. 2), 108°. The latter was rapidly deprived of its feathers, with the exception of the wings, whilst on the other they were left on. Both were suspended by the legs,

* I have occasionally found a delicate transparent membrane connecting some of the cancelli. Invariably the opening into the humerus is obstructed by the muscle attached to the cavity in which the foramen or foramina above mentioned are situated. Mr. Hunter found when the trachea of a cock was tied, and "the wing cut through the os humeri," the passage of air to the lungs was so difficult as to render it impossible for the animal to live longer than to prove that it breathed through the cut bone.—Observations on certain parts of the Animal Economy, p. 82.

the wings of the plucked fowl kept apart from the body, the wings of the other in close contact with the body. The room in which they were suspended was 53° at the time. From the great delicacy of the thermometer used, about a minute and a half sufficed *in recto* to give a good result; as the same instrument was used, the trial was made alternately as to time; in the first trial of the temperature beginning with No. 1, in the second with No. 2, and so on.

	h	m		°		°		°
March 29th.—	10	2	A.M., air	53	No. 1,	107·25	No. 2,	108
"	10	40	"	52	"	104	"	103
"	11	54	"	52	"	97	"	87
"	1		P.M., air	52	"	90	"	72
"	2	4	"	52	"	87	"	66
"	3	7	"	52	"	85	"	62
"	4	2	"	52	"	83	"	61
"	5	3	"	53	"	80	"	59·5
"	6	35	"	50	"	75·5	"	55·5
"	9	50	"	50	"	68	"	52
March 30th.—	12	15	A.M., air	48	"	65	"	50·5
"	8	30	"	49	"	55·5	"	48·5
"	10	20	"	51	"	55	"	49
"	12	15	P.M., air	53	"	54	"	50

From the last of these observations it is seen how little was the cooling effects from evaporation, the temperature of the plucked fowl rising a degree, and differing one degree only from the air of the room.

Of the other trials made, one was on a drake, one on a tawny owl.

The drake, well covered with feathers, weighed seven pounds. It was killed by drowning; the blood was retained. Like the fowls, it was suspended by the legs; its wings were apart. Previously its temperature *in recto* was 107°·5. The thermometer was left *in recto*.

	h	m		°		°
April 5th.—	10	51	A.M., air	55	Drake	107·5
"	11	25	"	55	"	104.
"	12	40	"	55	"	94.
"	2	8	"	55	"	89·5.
"	3	15	"	55	"	85.
"	4	35	"	55	"	81.
"	5	5	"	55	"	65.
"	11	45	"	53	"	65.
April 6th.—	8	30	A.M., air	51	"	57.

The owl was killed also by drowning. It had been fed the preceding evening. On the 2nd of December, when alive, at 10.30 A.M., its temperature *in recto* was 106°·5. Tho observations on its cooling were made on it placed on a table, the bird resting on its abdomen, the wings close to its sides; the thermometer was left *in recto*.

	h m		°		°
December 2nd.—	10 45	A.M., air	58	Owl	100
„	11 45	„	58	„	93·25
„	12 45	P.M., air	58	„	86·25
„	1 45	„	58	„	80·50
„	2 45	„	58	„	75·75
„	3 45	„	58	„	72
„	4 45	„	57	„	69·25
„	7 15	„	57	„	64·50
„	9 30	„	56	„	61·25
„	11 30	„	55	„	59·25
December 3rd.—	9	A.M., air	55	„	54·5*
„	12	M., air	58	„	56·25
„	2	P.M., „	60	„	57
„	4	„	60	„	58·5
„	12	„	57	„	57
December 4th.—	9	A.M., air	58	„	55·5†
„	4	P.M., air	60	„	58·5
„	12	„	57	„	57

These results seem sufficient to show that birds owe much of their high temperature, especially its preservation, to their clothing of feathers. Further, it may be remarked in proof of the little activity of their cuticular structure (except, indeed, in the growth of feathers), that birds are never observed to eat, or have their feathers wet from condensation on them of perspired moisture; nor am I aware that their breath becomes visible, to use a popular expression, in the coldest weather. And in accordance it would appear, comparing birds with animals of other classes, that the proportion of their aqueous element is somewhat less, which also harmonizes with an inconsiderable cooling effect from cutaneous evaporation—a fact which some of the results given seem to prove, and as is shown by the following, so far as trials on the dead are applicable, inferentially to the living animal.

Of four sparrows just shot, one (No. 1) weighing 414·5 grs. was suspended with its feathers entire; a second (No. 2) clipped, *i. e.* its feathers cut short, weighing 405·6 grs. (it had lost 23 grs. by the clipping), was suspended by its side, as were also the other two; No. 3, deprived of its feathers, its skin unbroken, weighing 414 grs.; No. 4, deprived of its skin as well as its feathers, weighing 384·5 grs. During twenty-four hours' exposure to the air of room varying from 48° to 50°, No. 1 lost 1·5 per cent.; No. 2, 2·3; No. 3, 7·9; No. 4, 17·4.

* In the room there was no fire from 5 P.M. on the 2nd to 8 A.M. on the 3rd; during the night the thermometer in air must have been below 54°·5; in a small cup of water close to the bird at 9 A.M. it was 53°.

† Water in cup *covered* 55°.

IV.—*Of the Kidneys and their Excretion.*

Another element in the problem of the temperature of birds is the kidneys, with th excretion. As is well known, these organs in birds are proportionally large and active; their secretion, not inconsiderable in quantity, and formed chiefly of urate of ammonia, is voided in a state far removed from the liquid, hardly semifluid from the little water it contains. Hence in the performance of the function there is but little loss of heat. Moreover, as it would appear from ultimate analysis that the urate contains less oxygen than urea, there must be a less expenditure of oxygen in its formation, leaving more for a more profitable conversion into carbonic acid.

What are the general conclusions which are admissible from the preceding results?

Do they not warrant the inference that the high temperature of birds is owing to a combination of circumstances, some positive, some negative; the one, the positive, acting through the air inspired and the conversion of oxygen into carbonic acid gas, productive of heat; the other, the negative conditions, such as those mentioned, influential mainly by economizing the heat when produced, or checking its escape?

Besides these negative conditions, it may be open to question, considering the proportional smallness of the lungs of birds, and the smallness of the nerves with which they are supplied, whether there are not other circumstances concerned of an ancillary kind—such, to enumerate some of the most probable, as a powerful heart, especially a powerful left ventricle; the quality of their blood, that but little viscid, as indicated by the little, if any tendency of the red corpuscles to collect in piles*; the large proportion of these corpuscles, and their nucleated structure, a structure with which may be connected an electrical influence.

If the chief use of the peculiar pneumatic system of birds be to secure a high temperature, it is probable, and is in part already admitted, that it may subserve other uses inferior only in degree of importance in relation to the habits and well-being of the class: for instance, as generally admitted, it may conduce to great power of flight in some, to running power in others, to vocal power in a third; and, in all, may not the thorough aëration of the blood, as denoted by its more florid hue even in the veins, be essential to the energy, to that intensity of action and endurance for which the muscles of those birds in which the structure under consideration is most developed, are so remarkable?

The subject in its entireness, it must be allowed, is full of interest. It affords in the variety of structure exhibited by different birds, supplementary to the lungs, ample scope for further research. Why some birds, such as the woodcock, the snipe, the swallow, birds of rapid and long flight, should be destitute of air in their bones; why the small birds, with few exceptions, the tits for instance, some of the smallest, should expe-

* In no instance have I seen the blood-corpuscles of any bird to cohere and form rouleaux or piles, nor have I seen a buffy coat on the blood of birds.

rience the same exemption; why one bird, the apteryx, a solitary example should be without air not only in every part of its osseous system, but also without air-sacs; and another bird, the grouse, not remarkable for power of flight, should have air in its femora as well as humeri, are questions which at present it may be difficult to answer, but which, it may be hoped, were careful and minute inquiry instituted, might be satisfactorily accounted for on the teleological principle of fitness of structure to use.

If I may be allowed to offer a conjecture, it seems to me probable that in our commonly received generalization relative to the consumption of oxygen in the respiration of birds, the quantity presumed to be used has been overrated, and that in many instances the expenditure of this gas may be found to be less proportionally than in the mammalia.

As supplementary to the preceding observations, I would beg to state some further particulars respecting birds, the results of the inquiry in which I have been engaged.

1st. *Of the birds examined.*—All of them were natives of the Lake District, with two or three exceptions which will be specified, and all were obtained between November and March, excepting those marked with an asterisk, which were shot in April and May. They may be divided into two sections, one including those birds in one or more of the bones of which air was found to exist communicating with the lungs. The other, of those birds in which in the corresponding bones no air could be detected. The birds were all at least one year old, an ample time, I apprehend, for the marrow which exists probably in the bones of every individual of the class at the time of hatching, and for some time after, to be absorbed in those in which it is not permanently present.

It may be right to remark that in every instance the question whether air was present or not was determined by an examination of the contents of the particular bones, and not merely from their appearance, which, as regards colour, is sometimes deceptive.

Of the birds belonging to the first section, the bones, which are named after each, were those only in which air was found, *i.e.* communicating with the lungs.

Section I.

	humeri,	scapulæ,	clavicles,	furcula,	femora.
Buzzard (*Falco buteo*)	humeri,	scapulæ,	clavicles,	furcula,	femora.
Tawny owl (*Strix stridula*)	do.	do.	do.	do.	do.
Carrion crow (*Corvus corone*)	do.	do.	do.	do.	—
Rook (*C. frugilegus*)	do.	do.	do.	do.	—
Jackdaw (*C. monedula*)	do.	do.	do.	do.	—
Magpie (*C. pica*)	do.	do.	do.	do.	—
Jay (*C. glandarius*)	do.	do.	do.	do.	—
Cuckoo (*Cuculus canorus*)	do.	—	—	—	—
Common fowl (*Gallus domesticus*)	do.	—	—	—	—
Pheasant (*Phasianus colchicus*)	do.	—	—	—	—

Grouse (*Tetrao scoticus*)	humeri,	scapulæ,	clavicles,	furcula,	femora.
Partridge (*Perdix cinerea*)	do.		—	—	—
Wood-pigeon (*Columba palustris*)	do.		—	—	—
Common pigeon (*C. domestica*)	do.		—		—
Wild duck (*Anas boschus*)	do.		—		—
Common duck (*A. domesticus*)	do.		—		—
Wigeon (*A. Penelope*)	do.	—	—	—	—
Skylark (*Alauda arvensis*)	do.	do.	do.	—	—
Woodlark (*A. arborea*)	do.	do.	do.	—	—
Great tit (*Parus major*)	do.	—	—	—	—
Blue tit (*P. cæruleus*)	do.	—	—	—	—
Marsh tit (*P. palustris*)	do.	—	—	—	—

Section II.

*Titlark (*Anthus pratensis*).
Tufted duck (*Anas fuligula*).
Common guillemot (*Uria Troile*).
Water-hen (*Gallinula chloropus*).
*Corncrake (*G. crex*).
Woodcock (*Scolopax rusticula*).
Snipe (*S. gallinago*).
Bar-tailed godwit (*S. ægocephala*).
Dunlin (*Tringa alpina*).
Little sandpiper (*T. pusilla*).
Missel-thrush (*Turdus viscivorus*).
Blackbird (*T. merula*).
Song-thrush (*T. musicus*).
Redwing (*T. iliacus*).
Fieldfare (*T. pilaris*).
Water-ouzel (*T. cinclus*).
Starling (*Sturnus vulgaris*).
Goldfinch (*Fringilla carduelis*).
Chaffinch (*F. cælebs*).
Siskin (*F. spinus*).
Lesser-redpole (*F. linaria*).
Common sparrow (*F. domestica*).
Mountain linnet (*F. montana*).
Robin (*Sylvia rubecula*).
*Stonechat (*S. rubicola*).
Wren (*S. troglodytes*).
*Hedge-warbler (*S. modularis*).
*Blackcap (*S. atricapilla*).
*Redstart (*S. phænicura*).
*Willow warbler (*S. trochilus*).
Bullfinch (*Loxia pyrrhula*).

*Greenfinch (*L. chloris*).
Yellowhammer (*Enteriza citrinella*).
Gray wagtail (*Motacilla boarula*).
Yellow wagtail (*M. flava*).
Common creeper (*Certhia familiaris*).
*Pied flycatcher (*Muscicapa atricapilla*).
*Spotted flycatcher (*M. grisola*).
*Swift (*Hirundo apis*).
*Swallow (*H. rustica*).
*Martin (*H. urbica*).
*Sand-martin (*H. riparia*).

Of the birds in each section, the crania, with some exceptions, contained air. The skull of the water-ouzel is one of the exceptions. It is not cellular like that of the majority, but compact and sinks in water. Its greater heaviness may be suitable to the habits of the bird, seeking its prey in the bed of ruuning streams with its head downmost. The same compactness of bone is seen in the crania of the Scolopacidæ. This compactness is remarkably contrasted with the cellular state of cranium of certain other birds, in which it is most strongly marked, where lightness as well as power of resistance is needed, such as that of the owls and tits.

There are certain bones which in the adult stage of the bird appear to be without both marrow and air; the scapular arch is occasionally an example of this, especially its posterior wing*, and also the sternum.

Professor Rudolph Wagner, in his 'Elements of Comparative Anatomy' (English translation), refers to the blackbird and thrush as instances of birds which have air in their femora. I have sought for air in these bones in all the thrushes I have examined, seven different species, but have found only marrow. If verified it would be a curious fact, that in one country air should occur in the bones in question and in another marrow.

Whether the circumstance of the presence or absence of air in the bones is deserving of attention in the classification of birds, may be worthy of the consideration of the naturalist. In all the tits I have examined, and the number has been considerable, especially of the blue tit, I have never found marrow in the humeri, and the same remark applies to these bones in the larks, but not to those of the pipits.

2nd. *Of the proportions of certain parts of birds as determined by weighing.*—In the Table which follows a statement is given of results, comprising the weight of the birds examined, of their feathers, and, with a few exceptions, of their bones, the latter after having been cleaned, deprived of their periostrum, and dried by exposure to the air†. In the first column the

* The posterior wing in the tabular list is designated scapula, the anterior portion, the coracoid process of some authors, is designated clavicle.

† In one instance (the bones of the buzzard) it was found, by weighing them before and after drying, that the difference or loss was 12·5 per cent.

weight in grains of the fresh birds, before the removal of their feathers, is inserted, the heading of the other columns is sufficiently distinctive. The primates of the wings and the quill-feathers of the tail in each instance were weighed apart. The weight of the whole of the plumage was ascertained by two weighings, one before, the other after the feathers had been taken out, the loss, including the whole of the quill-feathers, showing the total amount. When a trial was made of more than one of a species, the results have been inserted, on the idea that possibly they may be of some interest in relation to variations; these no doubt depending on many circumstances, such as sex, condition as to fatness, and others less easy of appreciation.

Species.	Sex.	Weight of bird.	Weight of tail-feathers.	Weight of wing-feathers.	Total weight of feathers.	Weight of bones.
		grs.	grs.	grs.	grs.	grs.
Buzzard	F.	23040	128	454	2276	1163·6
Buzzard	M.	12994	89	330·5	2022	1050·8
Tawny owl	M.	5776	22·6	127·6	696·2	428·5
Carrion crow	M.	7885	48·4	223·6	1074	556·9
Rook	F.	8664	45	207·3	1122·3	537
Rook	M.	6556	45·5	201·6	974	463·7
Jackdaw	M.	3900	28	98	390	272·5
Jay	—	2539	14·9	43·7	250·4	152·2
Cuckoo	M.	2091	23·6	49	197·6	106·9
Common fowl	F.	24851	57·5	214	1846·5	1438·7
Common duck	F.	22547	30	160·4	1755·5	
Woodcock	...	4198	8·1	58·9	306	280
Little sandpiper	...	629·4	1·7	8·8	62·7	36·6
Blackbird	...	1668	8·1	20·2	104·5	81·3
Song-thrush	...	1175	4	12	76·5	
Missel-thrush	...	2127	......	......	156	
Water-ouze	...	873	3	6·5	53·2	46·9
Skylark	F.	744	......	......	56·6	
Skylark	M.	657·7	......	......	53·2	33·2
Skylark	F.	643·7	2·8	12·2	64·7	
Skylark	F.	523	2·6	9·1	47·9	24·2
Woodlark	F.	493	2·3	9·1	47	24·2
Titlark	F.	368	1·5	5·4	14·5	20·2
Black-cap	M.	273·2	1·2	3·3	19·7	12·4
Stone-chat	M.	346·8	1·8	6·2	34·8	21·4
House-sparrow	...	421·7	2	5·7	30·6	25·3
Chaffinch	...	401	......	......	......	19
Bullfinch	M.	374	1·9	5·9	31·9	15·3
Greenfinch	M.	388·6	1·5	6	34·1	22
Swallow	F.	321·8	1·8	8·1	25·8	16
Swift	F.	784·3	9	18	55·3	39·5
Martin	F.	301	1·3	7	21	14·6
Sand-martin	F.	210·5	·9	6·2	14·5	9·4
Great tit	...	320·5	1·5	4	22	
Blue tit	...	180·9	·95	1·15	18·4	
Blue tit	M.	162·1	·83	2·16	16·49	8·71
Blue tit		145	......	......	15·4	
Blue tit		137	......	......	13·5	
Blue tit		167·5	......	......	13·8	

On the results in this Table I have but few remarks to offer. The regularity of feathers as to number, *i. e.* of the primates of the wings and of

the quill tail-feathers, is well known to the naturalist. In all the instances in which I have weighed the primates of each wing, I have found them, if not precisely of the same weight, to differ in the smaller birds not more than by ·1 or ·01 grain, and in the larger the difference has rarely exceeded 1 grain; a degree of equality this which might be expected, as essential to the regularity of flight; and small as it is, I am disposed to think that in the larger birds even it would hardly be appreciable could the quills be extracted with precisely the same proportion of adhering tissue.

Comparing the quill-feathers of the small with those of the large birds, the proportional weight of the latter, it would appear, is commonly greater than that of the former,—a disproportion, it may be inferred, connected with the larger birds having, as needed, stronger wing- and tail-quill feathers, and, indeed, stronger feathers generally, the few exceptions harmonizing, at least those of the common fowl and duck.

Comparing their bones, those of the larger and more powerful, as might also be anticipated, appear, too, proportionally heaviest.

Comparing individuals of the same species, whether as to the total weight of birds, or of feathers and bones, variations will be found to occur. The skylarks may be mentioned as examples, and also the tits, the former obtained not from the same locality, one having been procured from Lincolnshire, one from Oxford, a third from Yorkshire, a fourth from the immediate neighbourhood of Ambleside, where it is rarely seen, and where it came during a severe frost probably in quest of food, having been found close to a running stream*; the tits were all from the immediate neighbourhood of Ambleside.

3rd. *Of the weight of the principal bones of the skeleton.*—The results obtained are given in the next Table. The birds, the bones of which were the subjects of trial, have already been all mentioned, and may be identified by their weight, as inserted again in the first column. With the cranium, it may be stated, the maxillæ and facial bones were weighed, and with the pelvis the caudal vertebræ: the spine comprised all the other vertebræ excepting those anchylosed with the pelvis; the terminal bones of the extremities are designated by metacarpi, &c. for the upper, and by phalanges for the lower.

* In the gizzard of all the larks I have examined I have found grass, tending to prove that, at least in winter, meadow-grass is their chief food.

Species.	Sex.	Weight of birds.	Weight of cranium	Weight of os linguæ and hyoides.	Weight of spine.	Weight of pelvis.	Weight of sternum.	Weight of ribs.	Weight of furcula.	Weight of scapular arch.	Weight of humeri.	Weight of ulnæ and radii.	Weight of metacarpi.	Weight of femora.	Weight of Tibiæ.	Weight of tarsi.	Weight of phalanges.	Total weight.
		grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
Buzzard	F.	23040	86	2	51·6	88	36·5	24·8	10·1	44·5	125	223·7	85·6	73·4	141·6	81·6	89	1163·6
Buzzard	M.	12994	85·6	1·4	57·3	84·9	35·9	22·4	10·4	53·8	116	184·8	79·4	54·6	115·5	81	77·6	1050·8
Owl..................	M.	5776	61·5	1·1	25·8	25·4	12·5	10·4	2·4	16	37·5	60·6	28	33·5	47·6	32	34·2	428·5
Cuckoo	M.	2091	10·8	·3	7·5	8·3	6	2·3	1·5	7·4	12·1	18·1	12	5·8	7·6	3·2	4	106·9
Carrion crow	M.	7885	72	1·8	42·5	40·3	25·2	26·2	5·7	27·1	51·9	80·9	42·6	30	52	30·3	29	556·9
Rook	...	6556	56·6	3·6	36·5	30·1	17·7	12·4	5·3	21·5	41·7	74	42·6	26·3	48·8	26·4	20·4	463·7
Rook	...	8664	67·4	3·9	36·3	34·7	21·8	15·6	6	25·9	49·3	83·4	45·3	33·5	58·6	30·4	25	537·1
Jay	F.	2538	30·5	·6	13·3	10·8	5·2	3·9	1	7·5	11·5	18	8·4	9·8	16·7	10	5	152·2
Jackdaw	M.	3900	40·5	1·3	19	20	12·8	8·1	3	14·4	24·1	36·1	24·4	14·8	27·3	14·5	12·2	272·5
Common fowl......	F.	24851	57·4	1	121	195	97	51	11·4	77·6	101·2	100·8	60	156·8	217·6	115·3	74·8	1438·7
Woodcock	M.	4198	30·5	·3	19·4	23	22·5	5·3	4·1	16·2	39·9	32·4	20·5	20·2	25·6	11·1	9	280·1
Blackbird	M.	1668	10·4	·25	6·7	6·7	4·3	1·2	1	5·3	9·7	9	5·3	5·8	8·8	4·3	2·6	81·3
Skylark	M.	657	4·2	·1	2	2·8	2	1	·5	2·1	2·6	5·6	3·1	1·4	2·8	1·8	1·2	33·2
Woodlark	F.	493	3·6	·1	1·9	1·7	1·5	·8	·3	1·2	2·3	3·5	2·2	1	2	1·4	·7	24·2
Titlark	F.	368	2·6	·04	2·3	2·3	·8	·3	·2	1·28	1·86	2·2	1·4	·7	2	1·8	·5	20·2
Water-ouzel	...	873	6·3	·15	4·5	5·1	2·5	1·6	·6	2	4·2	4·2	2·4	2·2	5·1	3·4	1·8	46·9
Little sandpiper ...	...	629	4·2	·09	3	2·3	2·5	·8	·2	1·5	4·4	4·4	2·9	1·6	2·9	1·9	1·5	36·6
Yellow-hammer ...	...	365	3·4	·08	1	1·5	1·3	·5	·1	1	2·6	2·4	1·6	1	1·5	·8	·4	19·9
Bullfinch	M.	367	3·8	...	1·3	1·1	·8	·3	·2	1·8	1·8	1·7	1·2	·66	1·2	·75	·24	15·8
Greenfinch	M.	388	6·7	...	1·3	1·5	1·4	·6	·2	1·6	2	2	1·4	·8	1·3	·6	·4	22
Sparrow	M.	421	7·05	·14	2	1·7	1·4	·9	·3	·45	2·4	2·1	1·3	1·2	1·9	·8	·3	25·4
Sparrow	F.	428	5·5	·13	1·6	1·5	1·2	·8	·3	1·4	2	1·7	1·1	1·06	1·8	·9	·3	21·3
Stone-chat	M.	347	3·2	·1	1·5	1·5	1·1	·7	·3	1·4	2·3	2·8	1·6	1·1	2·2	1·2	·4	21·4
Blackcap............	M.	273	2·3	·03	1·1	1·1	·6	·2	·1	·65	1	1·2	·7	·65	1·3	·68	·2	12·48
Blue tit	M.	273	2·3	·03	·7	·7	·4	·2	·04	·4	·6	·85	·5	·45	·7	·46	·2	8·93
Swift	F.	784·3	4·2	·20	3·3	3·7	3·4	1·2	·50	2·4	3·7	4·30	6·8	1·70	2·4	·9	·8	39·5
Swallow	F.	321·8	1·9	·06	1·3	1·3	1	·8	·24	1·2	2·1	2·4	2·2	·5	·7	·3	·1?	16·1
Martin	F.	301	1·9	·04	1·15	1·15	1	·7	·23	1·2	2	2·1	1·8	·48	·6	·3	·05	14·6
Sand-martin	F.	201·5	1·35	·02	·6	·3	·65	·3	·14	·7	1·2	1·5	1·2	·3	·5	·2	·04?	9·48

The results given in the preceding Table may justify the remark,—a conclusion that might be anticipated, that generally the comparative weight of the bones of each species of birds bears a relation to the power exercised by the limbs on parts to which they belong; of this striking examples are afforded in the instances of the upper and lower extremities of the buzzard and common fowl; of the one, the buzzard, a bird of powerful flight, the wing-bones are proportionally the heaviest; whilst of the other, the fowl, which makes so little use of its wings and so much use of its legs, the opposite is the case; and other contrasts not less striking are noticeable.

Also, as might be anticipated, and in accordance with what was before observed of the feathers, the primates of each wing, the weight of the bones of each was found to be nearly the same, the difference being no greater than might be expected from the mode of preparing them.

4th. *Of the Composition of some of the principal bones.*—This was ascertained by calcination, by which merely the proportion of animal or combustible matter was determined and that of the incombustible, chiefly phosphate of lime. I shall first give the results of the trials on humeri and femora: in each instance the shafts of these bones were selected; and previously to a thorough drying over steam, they were deprived of their investing membrane internally as well as externally. Though the quantities employed did not exceed a few grains, and were even less than a grain from some of the smaller birds, yet as the weighing was carefully made to the one hundredth of a grain, the results may be received as tolerably reliable for comparison.

	Humerus.		Femur.	
	Phosphate of lime.	Animal matter.	Phosphate of lime.	Animal matter.
	grs.	grs.	grs.	grs.
Buzzard	69·53	30·47	68·80	31·20
Owl	68·50	31·50	71·20	28·80
Rook	69·20	30·80	70·70	29·30
Jackdaw	70·30	29·70	71·30	28·70
Grouse	67·70	32·30	71·40	28·60
Common fowl (cock two years old	70·22	29·78	71·40	28·60
Pigeon	74·70	25·30	73·40	26·60
Skylark	77·00	23·00	72·00	28·00
Blackbird	71·60	28·40	76·20	23·40
Water-ouzel	71·10	28·80	71·90	28·10
Sparrow	70·70	29·30	72·50	27·50
Woodcock	72·80	27·20	73·23	26·77
Guillemot	70·10	29·90	67·60	32·40

If inferences may be drawn from these results, they seem to favour the conclusion, first, that the proportion of phosphate of lime is somewhat greater in the bones of birds, the cylindrical of the extremities, than in the like bones of the Mammalia; and secondly, that the composition of those containing air and of those containing marrow is much the same, which is not

in accordance with a statement that has been made, that the former have a larger proportion of earthy constituents *.

It may generally be stated, I believe, that the composition and structure of each particular bone has relation to its function,—that where unyielding resistance is required, there, *cæteris paribus,* the proportion of phosphate of lime is largest, as witnessed in the majority of the long bones of the extremities; that where yielding and elasticity are needed, the proportion of animal matter is somewhat more considerable, as seen in the sternum, cranium, ribs, and maxillæ.

Also it may be generally stated, I believe, that in different species of the same family or genus of birds, the composition of corresponding bones varies comparatively little; and that where there is a variation, it too is connected with use, irrespective of size. And the same remark, I am disposed to infer, would be near the truth relative to the proportional weight of the bones in different species. The following results are offered in illustration.

First, of the cranium. The portion subjected to calcination was that covering the cerebrum.

	grs.		grs.	
Buzzard........	60·7	phosphate of lime,	39·3	animal matter.
Carrion crow......	59·5	„	40·5	„
Rook............	60·1	„	39·9	„
Jackdaw	59·5	„	40·5	„
Magpie	60·0	„	40·0	„
Owl	57·4	„	42·6	„
Common fowl	60·0	„	40·0	„
Common duck	60·0	„	40·0	„
Woodcock........	58·2	„	41·8	„
Skylark..........	63·0	„	47·0	„
Blue tit..........	58·0	„	42·0	„
Water-ouzel	57·5	„	42·5	„
Godwit, bar-tailed .	60·0	„	40·0	„
Dunlin	67·0	„	33·0	..

Of the above, some of the crania were cellular, others were compact, sinking in water. The crania of the owl and water-ouzel, as already mentioned, are extreme examples, and yet their proportion of phosphate of lime and animal matter is much the same, both conditions of bone, the very cellular structure of the one, and the compact structure of the other without cells, being suitable to the habits of the individual,—in the owl, great strength with lightness; in the water-ouzel, strength with a comparatively high specific gravity.

The following is the composition of the sternum of a few of the same birds, illustrative of the quality of lightness coupled with a considerable degree of yieldingness, which in the moist bone is very observable. The

* *Op. cit.* p. 69.

perpendicularity of the crest or keel of this bone, I may remark, is very characteristic of the equality of action of the great pectoral muscles attached to it.

	grs.	grs.
Buzzard..........	55 phosphate of lime,	45 animal matter.
Stork............	54 „	46 „
Carrion crow......	54 „	46 „
Jackdaw	55 „	45 „
Skylark..........	58 „	42 „

The composition of the maxillæ of a very few birds is given illustrative of the same quality. The lower jaw has been selected, and it has been divided, its anterior portion deprived of its horny integuments; its posterior, including its head, have been taken for the sake of comparison, the one being more elastic than the other.

		Phosphate of lime.	Animal matter.		Phosphate of lime.	Animal matter.
		grs.	grs.		grs.	grs.
Buzzard.	Anterior portion	64·8	35·2	Posterior	67·0	43·0
Song-thrush.	„	56·3	43·7	„	59·1	40·9
Carrion crow.	„	52·1	47·9	„	56·8	43·2
Godwit ...	„	64·8	35·2	„	67·0	33·0*

Here I would beg to offer a few remarks more on the subject of the bones of birds. It is stated, and by so high an authority as Professor Wagner, that their hollow bones are whiter than those filled with marrow. Generally this is a fact, and for the reason that, being translucent, the latter owe their colour to the marrow within them. Accordingly their colour varies with the colour of the marrow. Thus in some in which the marrow is of a light hue, almost white, as in the instance of the tawny owl, its ulnæ and radii are so white as to suggest their containing air. In another (the yellow-hammer), in which the marrow is of a bright yellow, as is also the fat, the long bones have the same hue. The same hue is seen in the bones of the cuckoo, and from the same cause, and also, but in a less degree, in those of the greenfinch. Nor are there wanting examples of a dark colour of the bones, from a dark colour of the marrow: those of the little sandpiper may be mentioned as an instance. Generally it may be remarked that the femora are darker than the humeri; and that the lower portion of the tibiæ is very much lighter than their upper, corresponding to the colour of the marrow in each. Another circumstance influencing the colour of the bones of birds is the degree of thickness of their walls. The thicker

* The bone of the under bill of the godwit, like that of the majority of the long-billed birds, is very slender and remarkably elastic, especially its anterior portion, that which is covered with integuments and a hard horny cuticle; the same portion is cellular and very vascular, suitable for renewing the growth of the beak as it is wasted in use, a remark more or less applicable to the beak of birds generally.

they are, the less translucent they are, and consequently the lighter is the colour, being less affected thereby by that of their contents. The long bones of the common guillemot, and also of the corn-crake, the parietes of which, especially of the wing-bones, are more than ordinarily thick, may be mentioned as illustrative examples.

In a preceding part of this paper I have referred to the size of some of the more important organs of birds. In many instances I have ascertained their weights. As examples, those of five different species are selected, and I give them without comment.

1. Tawny owl. Weight 5776 grs. April 7:	grs.
Brains, its membranes detached	139
Eye freed from muscle and fat	91
Lens (·58 inch diameter)	19·6
Skin freed from most of its fat	250
Membranous stomach	115
Liver without gall-bladder	154·7
Spleen	4·6
Pancreas	9·5
Kidneys	57·7
One lung, it contained a little coagulated blood	13·2
Heart freed from fat	39
Testes (no spermatozoa could be detected in them)	2·5
Great pectoral muscles	634
Other muscles of chest, those attached to furcula and scapular arch, exterior of costal	198
Muscles of humeri	188
Muscles of ulnæ and radii	156
Muscles of femora	272
Muscles of tibiæ	346
2. Rook. Weight 6556 grs. April 19.	
Brain freed from its membranes	118·6
Eye formed from muscle and fat*	41
Skin, exclusive of that of tarsi and phalanges, or very little fat adhering	344
Gizzard	208
Liver, gall-bladder detached	167
Spleen	2·8
Pancreas	23·2
Kidneys	72·6

* Of another male rook, shot April 29, and examined whilst still warm, the eye weighed 37·6 grs., the lens 1·2 gr.; it was very soft; evaporated to dryness it lost ·75 gr., or 62·5 per cent. of water. The lens of the rook, of that of which the weight of the organs is given above, was almost as liquid as the vitreous humour.

	grs.
One lung; it contained a very little clot	46
The other; it contained a little more *	49
Heart freed from fat	77
Testis, spermatozoa were abundant in its ducts	67
Glandula uropygii †	10·7
Great pectoral muscles	960
Muscles attached to scapular arch and bones of wings	493
Ditto, of lower extremities	680

3. Common fowl, a hen. Weight 24,851 grs.

Brain	53
Eye	30
One lung	53
The other; it contained a little clot	60
Gizzard	466
Spleen	27
Pancreas	36
Kidneys	181

4. Swallow, female. Weight 321·8 grs. May 5.

Brain	8
Heart	4·6
Liver	17·8
One lung	1·7
Stomach	8
Pancreas	·3
Spleen	·5
Kidneys	6·6
Great pectoral muscles	75

5. Cuckoo. Weight 2091 grs. May 3rd.

Brain	26·3
Eye	29
Lens	2·3
Liver	34·7
Spleen	·7
Pancreas	1·6
Kidneys	18
Stomach	33·2
Testis	1·3

* By steeping in water and expressing the blood the first was reduced to 32·7 grs., the second to 26·2 grs.

† The whitish semifluid expressed from it consisted of very minute oil-globules, and of the casts of the secreting tubes.

November 16, 1865.

Lieut.-General SABINE, President, in the Chair.

In accordance with the Statutes, notice of the ensuing Anniversary Meeting for the election of Council and Officers was given from the Chair.

Mr. Bowman, Dr. Frankland, Mr. F. Galton, Sir John Lubbock, and Mr. Spottiswoode, having been nominated by the President, were elected by ballot Auditors of the Treasurer's accounts on the part of the Society.

Mr. George Robert Gray was admitted into the Society.

The following communications were read :—

I. "Synthetical Researches upon Ethers.—Synthesis of Ethers from Acetic Ethers." By E. FRANKLAND, F.R.S., Professor of Chemistry in the Royal Institution of Great Britain, and in the Royal School of Mines, and B. F. DUPPA, Esq. Received July 13, 1865.

(Abstract.)

In a recent note * we have briefly described the synthesis of butyric and diethacetic ethers by acting consecutively upon acetic ether with sodium and the iodide of ethyl. In the present paper we have the honour to lay before the Royal Society the detailed results of one section of this research, embracing the action of sodium and the iodides of methyl, ethyl, and amyl upon acetic ether.

I. *Action of Sodium and Ethyl Iodide upon Acetic Ether.*

When acetic ether is treated with sodium at a temperature gradually rising to 120° C., hydrogen is evolved, and a thick brownish liquid produced; the latter solidifies on cooling to a yellowish mass, presenting the appearance of beeswax. On digesting this solid mass with ethylic iodide at 100° C. for several hours, a number of products are formed, which on the addition of water, may be distilled off from a residue consisting chiefly of iodide of sodium. The distillate can readily be separated into an aqueous and an oily portion. The latter then presented the appearance of a light straw-coloured oil, possessing a pleasant and fragrant odour. It was washed and then dried over chloride of calcium, and submitted to fractional distillation, by which traces of alcohol, acetic ether, and ethyl iodide were effectually removed from the other products, which now boiled between 120° and 265° C. We have described the constituents of this complex liquid under two distinct heads, viz. :—

1st. Products depending upon the duplication of the atom of acetic ether.

2nd. Products derived from the replacement of hydrogen in the methyl of acetic ether by the alcohol-radicals.

In order successfully to separate the two products from each other, and

* Proceedings of the Royal Society, vol. xiv. p. 198.

especially to disentangle their constituent compounds, it is absolutely necessary to operate upon large quantities of material. But if this be done, there is obtained a considerable quantity of the products of the first division boiling between 204° and 208°, whilst the products of the second division boil considerably below these temperatures.

a. Examination of the products depending upon the duplication of the atom of acetic ether.

Submitted to analysis, this liquid was found to consist of two bodies of the formulæ

$$C_{10}\,H_{18}\,O_3, \text{ and } C_8\,H_1\,O_{43},$$

separable from each other by repeated rectification, and also by the action of boiling aqueous potash, which decomposes the second but scarcely affects the first.

From the results of the analysis, and from considerations which are fully entered into in the paper, we propose for these bodies the following names and formulæ:

$$\text{Ethylic diethacetone carbonate} \;..\; C_4\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline (C_2\,H_5)_2 \\ \hline O'' \\ O\,C_2\,H_5 \end{array}\right.$$

$$\text{Ethylic ethacetone carbonate} \;....\; C_4\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline C_2\,H_5 \\ H \\ \hline O'' \\ O\,C_2\,H_5 \end{array}\right.$$

The production of ethylic diethacetone carbonate is explained in the following equations:

$$\underset{\text{Acetic ether.}}{2C_2\left\{\begin{array}{l} H_3 \\ \hline O'' \\ O\,C_2\,H_5 \end{array}\right.} + Na_2 = C_4\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline Na_2 \\ \hline O'' \\ O\,C_2H_5 \end{array}\right. + \underset{\text{Alcohol.}}{\left.\begin{array}{c} C_2\,H_5 \\ H \end{array}\right\}O} + H_2$$

$$C_4\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline Na_2 \\ \hline O \\ O\,C_2H_5 \end{array}\right. + 2C_2\,H_5\,I = \underset{\text{Ethylic diethacetone carbonate.}}{C_4\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline (C_2\,H_5)_2 \\ \hline O'' \\ O\,C_2\,H_5 \end{array}\right.} + 2Na\,I$$

Ethylic diethacetone carbonate is a colourless and somewhat oily liquid, possessing a fragrant odour and a pungent taste. It is insoluble in water, but miscible in all proportions with alcohol and ether. Its specific gravity is ·9738 at 20° C. It boils between 210° and 212°, and distils unchanged.

The density of its vapour was found to be 6·59. The above formula, corresponding to two volumes, requires the number 6·43. Boiling aqueous solutions of potash and soda have scarcely any action on ethylic diethacetone carbonate, but baryta-water and lime-water decompose it with great facility, as do also boiling alcoholic solutions of potash and soda. In all cases a carbonate of the base is precipitated, and alcohol, together with a light ethereal liquid, is separated.

This liquid, freed from alcohol by repeated washing with salt and water, boiled, after drying over chloride of calcium, between 137°·5 and 139° C. Submitted to analysis, it yielded results corresponding with the formula

$$C_7\,H_{14}\,O.$$

We regard this body as *diethylated acetone.* Its formula and its relations to acetone may be thus expressed:

$$\underset{\text{Acetone.}}{\left\{\begin{array}{l} C\,H_3 \\ C\,Me\,O \end{array}\right.} \qquad \underset{\text{Diethylated acetone.}}{\left\{\begin{array}{l} C\,Et_2\,H \\ C\,Me\,O \end{array}\right.}$$

Diethylated acetone is produced from ethylic diethacetone carbonate by the action of alcoholic potash according to the following equation:

$$\underset{\substack{\text{Ethylic diethacetone} \\ \text{carbonate.}}}{C_4\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline (C_2\,H_5)_2 \\ \hline O'' \\ O\,C_2\,H_5 \end{array}\right.} + 2K\,H\,O = \underset{\substack{\text{Potassium} \\ \text{carbonate.}}}{C\left\{\begin{array}{l} O \\ O\,K \\ O\,K \end{array}\right.} + \underset{\text{Alcohol.}}{C_2\,H_5\,O\,H} + \underset{\substack{\text{Diethylated} \\ \text{acetone.}}}{C_3\left\{\begin{array}{l} H_3 \\ \hline O'' \\ \hline (C_2\,H_5)_2 \\ \quad H \end{array}\right.}$$

Diethylated acetone is a colourless, transparent and mobile liquid, possessing a penetrating odour of camphor, and the burning and bitter after-taste of the same substance. It is very slightly soluble in water, but miscible in all proportions with alcohol and ether. Its specific gravity is ·8171 at 22° C. It boils at 137°·5 to 139° C. A determination of its vapour-density gave the number 3·86, the above formula requiring 3·93. Diethylated acetone does not oxidize in the air, neither does it reduce ammoniacal solution of nitrate of silver when boiled with it. Mixed with concentrated solution of sodium bisulphite, it forms an oily compound which scarcely exhibits signs of crystallization at 0° C. It suffers no alteration by prolonged boiling with alcoholic potash. It is isomeric with butyrone, with a ketone obtained by Fittig * in the distillation of calcium valerianate, and with œnanthol. From the first it is distinguished by its lower boiling-point (138°), butyrone boiling at 144° C., and Fittig's ketone at 161° to 164°, and from the third by its different properties, which are essentially those of a ketone and not of an aldehyde. The difference in structure of three of these bodies may be expressed with considerable certainty by the following formulæ:

$$\underset{\text{Diethylated acetone.}}{\left\{\begin{array}{l} C\,Et_2\,H \\ C\,Me\,O \end{array}\right.} \qquad \underset{\text{Butyrone.}}{\left\{\begin{array}{l} C\,Et\,H_2 \\ C\,(Pr)\,O \end{array}\right.} \qquad \underset{\text{Œnanthol.}}{\left\{\begin{array}{l} C\,(Aq)\,H_2 \\ C\,H\,O \end{array}\right.}$$

* Ann. Ch. Pharm., vol. cxvii. p. 68.

Ethylic ethacetone carbonate is produced by the action of sodium and ethylic iodide upon acetic ether, according to the following equations:

$$\underset{\text{Acetic ether.}}{C_2\left\{\begin{array}{l}H_3\\ O''\\ O\,C_2\,H_5\end{array}\right.} + Na = \underset{\text{Ethylic sodacetone carbonate.}}{C_4\left\{\begin{array}{l}H_3\\ O''\\ \overline{Na}\\ H\\ \overline{O''}\\ O\,C_2\,H_5\end{array}\right.} + \underset{\text{Alcohol.}}{C_2\,H_5\,O\,H} + H$$

$$\underset{\text{Ethylic sodacetone carbonate.}}{C_4\left\{\begin{array}{l}H_3\\ \overline{O''}\\ \overline{Na}\\ H\\ \overline{O'}\\ O\,C_2\,H_5\end{array}\right.} + C_2\,H_5\,I = \underset{\text{Ethylic ethacetone carbonate.}}{C_4\left\{\begin{array}{l}H_3\\ \overline{O''}\\ \overline{C_2\,H_5}\\ H\\ \overline{O''}\\ O\,C_2\,H_5\end{array}\right.} + Na\,I.$$

Ethylic ethacetone carbonate is a colourless and transparent liquid, possessing a very fragrant odour and an aromatic taste. It is nearly insoluble in water, but miscible in all proportions with alcohol and ether. Its density in the liquid condition is ·9834 at 16° C. It boils at 195° C., and distils without decomposition. A determination of its vapour-density gave the number 5·36. The above formula requires 5·45. Ethylic ethacetone carbonate is readily attacked by boiling aqueous solutions of potash and soda, yielding carbonates of these bases, alcohol, and *ethylated acetone*, according to the following equation:

$$\underset{\text{Ethylic ethacetone carbonate.}}{C_4\left\{\begin{array}{l}H_3\\ \overline{O''}\\ \overline{C_2\,H_5}\\ H\\ \overline{O''}\\ O\,C_2\,H_5\end{array}\right.} + 2K\,H\,O = C\left\{\begin{array}{l}O''\\ O\,K\\ O\,K\end{array}\right. + \underset{\text{Alcohol.}}{C_2H_5\,O\,H} + \underset{\text{Ethylated acetone.}}{C_3\left\{\begin{array}{l}H_3\\ \overline{O''}\\ \overline{C_2\,H_5}\\ H_2\end{array}\right.}$$

Ethylic ethacetone carbonate is still more readily decomposed by aqueous solution of baryta and by alcoholic potash, in both cases ethylated acetone and a carbonate of the base is produced.

Ethylated acetone may be freed from alcohol by repeated washing with salt and water, but it is best obtained in a state of absolute purity by combination with, and subsequent separation from, bisulphite of soda. Ethylated acetone thus purified and rectified from quicklime yielded on analysis numbers agreeing well with the formula

$$C_5\,H_{10}\,O,$$

which may be reduced to the radical type as follows:

$$C_3\left\{\begin{array}{l}\underline{H_3}\\ \underline{O''}\\ C_2H_5\\ H_2\end{array}\right.=C_2\left\{\begin{array}{l}CH_3\\ \underline{O''}\\ C_2H_5\\ H_2\end{array}\right.=\left\{\begin{array}{l}C\,Et\,H_2\\ C\,Me\,O\end{array}\right.$$

Its relations to acetone and diethylated acetone are then clearly seen in the following formula,

$\left\{\begin{array}{l}CH_3\\ C\,Me\,O\end{array}\right.$	$\left\{\begin{array}{l}C\,Et\,H_2\\ C\,Me\,O\end{array}\right.$	$\left\{\begin{array}{l}C\,Et_2\,H\\ C\,Me\,O\end{array}\right.$
Acetone.	Ethylated acetone.	Diethylated acetone.

Ethylated acetone is a colourless, transparent and very mobile liquid, possessing a powerful and pleasant odour, in which that of camphor is slightly perceptible. Its specific gravity is ·8132 at 13° C., and ·8046 at 22° C. It boils steadily at 101°·5, and its vapour has the density 2·951, the above formula requiring 2·971. Ethylated acetone neither absorbs oxygen from the air, nor reduces ammoniacal solutions of silver. It yields with concentrated solutions of bisulphite of soda a compound in large and brilliant crystals, which are quite permanent in the air, and which at once distingush it from diethylated acetone, the latter producing under the same circumstances an oily compound. Ethylated acetone is not altered by prolonged ebullition with alcoholic potash.

β. Examination of the products derived from the replacement of hydrogen in the methyl of acetic ether by ethyl.

The chief results of this examination are given in the note above alluded to *, and we have only to add that ethacetic acid is identical with butyric acid, whilst diethacetic acid is isomeric with caproic acid.

II. *Action of Sodium and Methylic Iodide upon Acetic Ether.*

This reaction is conducted in substantially the same manner as that above described, and the products are completely homologous. Thus there are produced two carboketonic ethers, and an ether derived from acetic ether by the substitution of methyl for hydrogen. The latter has been already described in our previous communication on this subject.

The following are the names and formulæ of the carboketonic ethers:—

$$\text{Ethylic dimethacetone carbonate}\,..\,C_4\left\{\begin{array}{l}\underline{H_3}\\ \underline{O''}\\ \underline{(C\,H_3)_2}\\ O''\\ O\,C_2H_5\end{array}\right.$$

$$\text{Ethylic methacetone carbonate}\,..\,C_4\left\{\begin{array}{l}\underline{H_3}\\ \underline{O''}\\ C\,H_3\\ \underline{H}\\ O''\\ O\,C_2H_5\end{array}\right.$$

* Proceedings of Royal Society, vol. xiv. p. 198.

The reactions involved in the production of these bodies are exactly similar to those by which the corresponding ethylic bodies are formed.

Ethylic dimethacetone carbonate is a colourless, slightly oleaginous liquid, possessing a peculiar penetrating and pleasant odour, and a sharp burning taste. It is scarcely at all soluble in water, but readily so in alcohol and ether. Its specific gravity is ·9913 at 16° C. It boils constantly at 184° C., and distils unchanged. A determination of its vapour-density gave the number 5·36, the above formula requiring 5·45. Its remaining properties very closely resemble those of ethylic diethacetone carbonate. Boiled with baryta-water, it gives barium carbonate and *dimethylated acetone*,

$$\left\{\begin{array}{l}\mathrm{C\,Me\,H_2}\\ \mathrm{C\,Me\,O}\end{array}\right.$$

Dimethylated acetone is a colourless, transparent and very mobile liquid, possessing a pleasant odour, reminding at the same time of parsley and acetone. Its specific gravity is ·8099 at 13° C., and it boils at 93°·5 C. Its vapour-density is 2·92, theory requiring 2·97. Dimethylated acetone closely resembles its ethylic homologue in all its chemical properties; like diethylated acetone, it is oxidized with difficulty, and does not very readily form a crystalline compound with bisulphite of soda—differing in the latter respect markedly from its isomer, ethylated acetone, and also from methylated acetone described below.

Ethylic dimethacetone carbonate and ethylic methacetone carbonate boil at the same temperature, and cannot therefore be separated by rectification; but we have prepared and examined the ketone from the second of these bodies; viz. *methylated acetone,* which has the formula

$$\left\{\begin{array}{l}\mathrm{C\,Me\,H_2}\\ \mathrm{C\,Me\,O}\end{array}\right.$$

Methylated acetone is best obtained in a state of purity by combining it with bisulphite of soda, pressing the beautiful crystalline compound so formed between folds of blotting-paper to remove traces of dimethylated acetone, exposing it over sulphuric acid *in vacuo,* and then regenerating the methylated acetone by distillation with aqueous potash. The liquid so obtained, after drying over quicklime and rectification, gave analytical results corresponding with the above formula.

Methylated acetone is a colourless, transparent and very mobile liquid, possessing an odour like chloroform, but more pungent. It is tolerably soluble in water, and more than slightly so in a saturated solution of common salt. Its specific gravity is ·8125 at 13° C. It boils at 81° C., and its vapour-density is 2·52, the above formula requiring 2·49. Methylated acetone is identical with the ethyl-acetyl obtained by Freund * in acting upon chloride of acetyl with zinc ethyl. Methylated acetone forms a splendidly crystalline compound with bisulphite of soda, and in its other chemical properties so closely resembles ethylated acetone as to require no further description. It retains alcohol with such tenacity as to render its separation from that liquid by washing and treatment with chloride of cal-

* Ann. Ch. Pharm., vol. cxviii. p. 1.

cium almost impossible. This separation, however, is readily effected by bisulphite of soda.

III. *Action of Sodium and Amyl Iodide upon Acetic Ether.*

For this reaction the compounds of sodium derived from acetic ether were prepared as before, and were then submitted to the action of amylic iodide for several hours at the boiling-point of the mixture. When the sodium had all become converted into iodide, water was added and the supernatant liquid decanted. We reserve a complete description of this liquid for our next communication, and will here confine ourselves to the separation from it of œnanthylic acid, which was obtained as follows:—The crude product, after drying over chloride of calcium, was submitted to rectification, and the portion boiling between 170° and 190° C. collected apart and decomposed by ebullition with alcoholic potash. By this treatment we destroyed any ethylic amylacetone carbonate and ethylic diamylacetone carbonate that were present, and obtained a potash-salt of an acid derived from acetic acid by the substitution of one atom of amyl for one of hydrogen. The potash-salt thus obtained was distilled with excess of sulphuric acid diluted with a large quantity of water. Upon the distillate there floated an oily acid, possessing an odour resembling œnanthylic acid. This acid was converted into an ammonia-salt, from which a silver-salt was prepared by precipitation. After being well washed with cold water, this salt yielded numbers on analysis closely corresponding with the formula of amylacetate or œnanthylate of silver:

$$C_2\left\{\begin{array}{l} C_5\,H_{11} \\ H_2 \\ \hline O'' \\ O\,Ag \end{array}\right.$$

We have also examined the barium-salt, which is an amorphous soapy substance. Dried at 100° C., ·2715 grm. gave ·1599 grm. of barium sulphate, corresponding to 34·62 per cent. of barium. Barium œnanthylate contains 34·69 per cent. of barium. We believe amylacetic acid to be identical with œnanthylic acid.

The concluding portion of the paper is devoted to a discussion of the theoretical bearings of the reactions above described, and to the investigation of the internal architecture of the synthetically prepared ethers, acids, and ketones.

II. "Researches on the Hydrocarbons of the Series $C_n\ H_{2n+2}$."—No. II. By C. Schorlemmer, Esq., Assistant in the Laboratory of Owens College, Manchester. Communicated by Prof. H. E. Roscoe. Received July 20, 1865.

From my experiments communicated to the Royal Society on the 6th of April, 1865, I concluded that the question, whether only one series of hydrocarbons of the general formula $C_n\ H_{2n+2}$ exists, or whether this

series exhibits cases of absolute isomerism, can only be definitely decided by obtaining from different sources perfectly pure hydrocarbons, having the same composition. But unfortunately only a few of the hydrocarbons can be obtained perfectly pure, and still fewer of these possessing the same composition can be derived from different sources. This is seen by a glance at the following Table, containing those alcohol-radicals and hydrides which have been obtained with certainty in a pure state.

		Boiling-points.		Boiling-points.
$C_2 H_6$	Methyl.		Hydride of ethyl.	
$C_4 H_{10}$	Ethyl.		——	
$C_5 H_{12}$	——	°	Hydride of Amyl	° 30
$C_6 H_{14}$	Ethyl-butyl	62	Hydride of hexyl * ..	69·5
$C_7 H_{16}$	Ethyl-amyl..........	90	Hydride of heptyl † ..	99
$C_8 H_{18}$	Butyl..............	108	——	
$C_9 H_{20}$	Butyl-amyl	132	——	
$C_{10} H_{22}$	Amyl	158	——	
$C_{12} H_{26}$	Hexyl (caproyl)	202	——	

For the purpose of examining the question of the identity or the isomerism of these hydrocarbons, I selected methyl-hexyl and hydride of heptyl, obtained from azelaic acid, comparing the properties of these bodies with ethyl-amyl, as described in my last communication.

(1) *Methyl-hexyl.*

Methyl-hexyl (methyl-caproyl) has already been prepared by Wurtz by the electrolysis of a mixture of acetate and œnanthylate of potassium, but he has obtained it in a small quantity only, and in a very impure state ‡. I adopted the same method, and am able to confirm all that Wurtz has stated. Although I employed several ounces of œnanthylate of potassium, only a very inconsiderable quantity of an aromatic oil was obtained, which, in order to isolate the hydrocarbon $C_7 H_{16}$, was first distilled with concentrated sulphuric acid, by the action of which sulphurous acid was evolved and a black charry matter separated out. The oily distillate was well washed and further purified by means of nitric acid, caustic potash, and sodium, as described in my former papers, and then the small quantity of methyl-hexyl separated by fractional distillation from hexyl, $C_{12} H_{26}$, which latter hydrocarbon is formed in by far the greatest proportion.

Methyl-hexyl boils at 89°–92° C., and has the specific gravity 0·6789 at 19° C. The analysis gave the following numbers:—

0·2002 substance gave 0·6150 carbonic acid and 0·2900 water.

	Calculated.	Found.
C_7	84	83·78
H_{16}	16	16·14
	100	99·92

* Dale, Journ. Chem. Soc. New Ser. ii. p. 258. † Ibid.

‡ Ann. de Chim. et de Phys. 3 sér. xliv. 296.

The quantity which I obtained was only sufficient for determining the boiling-point and the specific gravity, both of which nearly coincide with those of ethyl-amyl; and although I could not investigate its reactions, I believe that these also will agree with those of ethyl-amyl, so that the two hydrocarbons appear to be identical.

(2) *Hydride of Heptyl from Azelaic Acid.* By C. Schorlemmer and R. S. Dale, B.A.

One of us has shown that by heating a mixture of azelaic acid and caustic baryta to a dull red heat, an aromatic liquid is obtained, which chiefly consists of the hydrocarbon $C_7 H_{16}$. By oxidizing castor-oil with nitric acid on a large scale, one pound of pure azelaic acid was prepared, which yielded about one ounce of a hydrocarbon boiling between 95° and 100°. Subjected to fractional distillation, a small quantity of hydride of hexyl from the suberic acid, which still adhered to the azelaic acid, was separated, and now the liquid boiled constantly at 100°·5 C. (corrected). The sp. gr. at 20°·5 C. was found to be 0·6840.

The determination of its vapour-density gave the following results:—

Balloon + air....................	7·5660
Temperature of air	16°·5
Balloon and vapour..............	7·7830
Temperature on sealing	140°
Capacity of balloon..............	115·5 cub. centims.

Vapour density calculated.	Found.
3·46	3·63

This hydrocarbon is very easily attacked by chlorine, the chloride $C_7 H_{15} Cl$ being chiefly formed, together with a small quantity of higher chlorinated products.

The chloride boils at 151°–153° C., and has the specific gravity 0·8737 at 18°·5. It is a colourless liquid, smelling exactly like the chloride obtained from ethyl-amyl.

0·3045 substance gave 0·3165 chloride of silver and 0·0045 of metallic silver.

Calculated.	Found.
26·40 per cent. Cl	26·20 per cent Cl

By heating this chloride with acetic acid and acetate of potassium in sealed tubes, heptylene and acetate of heptyl are formed. This decomposition goes on much quicker than in the case of the chloride from ethyl-amyl; and the proportions of the substances formed also differ, as only a very small quantity of heptylene is produced, and the chief product consists of the acetate, whilst the chlorides from ethyl-amyl and from petroleum yield these two substances in about equal quantities.

The heptylene boils at 95°–97°, and has the specific gravity 0·7026 at 19°·5.

The faint garlic-like smell is identical with that of the heptylene described in my last paper.

0·1952 substance gave 0·6130 carbonic acid and 0·2510 water.

	Calculated.		Found.
C_7	84	85·7	85·65
H_{14}..........	14	14·3	14·28
	8	100·0	99·93

The acetate also has the same pear-like smell as the acetate from ethyl-amyl. It boils at 180°–182°, and has the specific gravity of 0·8605 at 16°.

0·2446 substance gave 0·6135 carbonic acid and 0·2540 water.

	Calculated.		Found.
C_9	108	68·35	68·40
H_{18}..........	18	11·39	11·53
O_2	32	20·26	——
	158	100·00	

From the acetate the alcohol was prepared by heating with a concentrated solution of caustic potash. Dried over caustic baryta, the alcohol boiled at 164°–167°.

The specific gravity at 19°·5=0·8286.

Its odour cannot be distinguished from that of the alcohol from ethyl-amyl.

By oxidizing it with chromic acid, first the odour of œnanthol is perceived, and then an oily acid is obtained, which by its smell, as well as the analysis of its silver-salt, was recognized as œnanthylic acid.

0·1205 of the silver-salt obtained by saturating the rectified acid distillate with carbonate of silver, gave 0·0551 of metallic silver, or 45·72 per cent., the formula $C_7 H_{13} Ag O_2$ requiring 45·57 per cent. Ag.

The annexed Table gives the boiling-points and specific gravities of the hydrocarbons $C_7 H_{16}$ of different origin, and their derivatives. From these data, as well as from the experiments detailed in this and in my former papers, it appears that we meet here with examples of absolute isomerism, viz. compounds having the same percentage composition and the same constitutional formula (A. Crum Brown), but differing from each other in their physical properties. This is not only the case with the hydrocarbons, but also, in a greater or less degree, with their derivatives.

Ethyl-amyl and hydride of heptyl from azelaic acid, as well as the corresponding chlorides, were obtained in as pure a state as possible, and in pretty large quantities; and although only small quantities of the acetate, alcohol, and olefine from the hydride were at our disposition, yet the greatest care was taken to obtain them pure, and all determination of the boiling-points and specific gravities were carried out under the same circumstances, the same thermometer always being used, so that they may be fairly compared with each other.

		Heptyl compounds derived from			
		1. Petroleum.	2. Ethyl-amyl.	3. Azelaic acid.	4. Methyl-hexyl.
$C_7 H_{16}$	Boil.-point	90°–92°(98–99°)	90°–91°	100°·5	89°–92°
	Sp. gravity	0·7148 at 15°	0·6795 at 20°	0·6840 at 20°·5	0·6789 at 19°
$C_7 H_{14}$	Boil.-point	95°–97°	93°–95°	95°–97°	
	Sp. gravity	0·7383 at 17°·5	0·7060 at 12°·5	0·7026 at 19°·5	——
$C_7 H_{15} Cl$	Boil.-point	148°–150°	146°–148°	151°–153°	
	Sp. gravity	0·8965 at 19°	0·8780 at 18°·5	0·8737 at 18°·5	——
$C_7 H_{16} O$	Boil.-point	164°–165°	163°–165°	164°–167°	
	Sp. gravity	0·8479 at 16°	0·8291 at 13°·5	0·8286 at 19°·5	——
$\left.\begin{matrix} C_7 H_{15} \\ C_2 H_3 O \end{matrix}\right\} O$	Boil.-point	179°–181°	178°–180°	180°–182°	——
	Sp. gravity	0·8865 at 190°	0·8707 at 16°·5	0·8605 at 16°	——

C. M. Warren has lately published * an investigation on the hydrocarbons contained in the American petroleum, which he isolated according to a new method of fractional condensation. He states that the petroleum contains two series of the hydrocarbons $C_n H_{2n+2}$, the isomeric pairs of which show a difference in their boiling-points of 7°–8°.

Some of the results which I have formerly obtained tend to confirm this view.

Frankland, Wurtz, Pelouze, and Cahours found 30° as the boiling-point of hydride of amyl; the hydrocarbons of the same composition, which I isolated from the light oils obtained from Cannel coal, boils constantly between 39° and 40°. The hydride of heptyl obtained from the same source boiled at 98°–99°, and the same hydrocarbon I found in American petroleum, whilst Pelouze and Cahours give 92°–94° as the boiling-point; and in my last communication I have quoted some experiments made by Mr. Wright, who found that from that part of American petroleum which boils between 95°–100° a considerable quantity of a hydrocarbon, $C_7 H_{16}$, may be obtained which boils constantly at 90°–92°. These latter hydrocarbons and their derivatives show, even after repeated rectification, higher specific gravities than the isomeric alcohol-radicals and the hydrocarbons from azelaic acid.

Thus it appears that bodies showing a purely physical isomerism are as numerous in the marsh-gas family as in the case of the terpines, $C_{10} H_{16}$.

In order to complete this investigation, I intended to study in the same manner the hydrocarbons $C_6 H_{14}$, namely hydride of hexyl from suberic acid, methyl-amyl, and, if possible, ethyl-butyl; but this intention could not be carried out, as I could not succeed in preparing methyl-amyl. This hydrocarbon appears not to be formed by any of the methods which are employed to prepare the so-called mixed alcohol-radicals. A mixture of the iodides of methyl and amyl is exceedingly slowly attacked by sodium. The boiling-point of the mixture is below the fusing-point of sodium, and the metal soon becomes coated with a hard crust of iodide of sodium. I

* Mem. American Academy, New Series, vol. ix. p. 156.

added, therefore, a sufficient quantity of pure amyl to raise the boiling point, but even the sodium in the fused state acts very slowly, a considerable quantity of gaseous products being evolved.

After the mixture had been heated for a week, large quantities of the iodides were still present, and after destroying these by strong nitric acid, the remaining hydrocarbon was found to be pure amyl.

No better results were obtained by adding anhydrous ether to the mixture. The action in the cold is exceedingly slow; heated in sealed tubes, the iodides are soon decomposed; but besides gaseous products, only amyl, and not a trace of a mixed radical, is formed.

Besides hydride of heptyl, other products are formed by the action of caustic baryta upon azelaic acid. Of those only one could be obtained in a pure state. If the aromatic liquid which is first obtained is distilled with water, hydride of heptyl chiefly distils, and a brown oily liquid remains behind, which, after cooling, solidifies to a crystalline mass containing a brown aromatic oil which may be removed from the crystals by pressing between blotting-paper. The solid substance is repeatedly recrystallized from hot diluted alcohol, in which the still adhering oil is very slightly soluble. The pure substance is thus obtained in small colourless needles, which are grouped in tufts. It is odourless and tasteless, very soluble in ether and in alcohol, insoluble in water, melts between 41° and 42°, solidifies again at 40°, and distils between 283°–285° (not corrected) without decomposition.

The following analysis shows that it has the formula $C_n H_{2n}$.

0·2480 substance gave 0·7800 carbonic acid and 0·3215 water.

	Calculated.	Found.
C_n	85·7	85·77
H_{2n}	14·3	14·40
	100·0	100·17

The quantity obtained was not sufficient to determine the vapour-density.

If this olefine is suspended in water and bromine added, not in excess, the two substances combine readily to a colourless oily liquid, the odour of which resembles bibromide of ethylene. It cannot be distilled without decomposition, and appears even to be decomposed by a diluted solution of caustic soda, as a small portion thus treated in order to remove an excess of bromine changed its odour completely. By an unfortunate accident the whole of the bromide was lost, with the exception of the portion treated with caustic soda. This was washed with water, dissolved in ether, and the ethereal solution dried with chloride of calcium. After evaporating the ether and drying the remaining small quantity of heavy yellow oil over sulphuric acid under the air-pump, only just sufficient was left to determine the bromine.

0·1500 gave 0·0736 of bromide of silver, and 0·0102 of metallic silver, corresponding to 25·8 per cent. of bromine.

From the boiling point of the olefine it appears that its molecular for-

mula is most likely $C_{16}H_{32}$; and if bromine forms the bibromide, $C_{16}H_{32}Br_2$, from which by the action of caustic soda H Br is abstracted, the compound analyzed would be $C_{16}H_{31}Br$, which formula requires 26·4 per cent. of bromine, whilst the analysis gave 25·8 per cent.; the hydrocarbon would then be an isomer of cetene.

III. "On the Laws of Connexion between the conditions of a chemical Change and its Amount." By A. VERNON HARCOURT and W. ESSON. Communicated by Sir B. C. BRODIE, Bart., F.R.S. Received September 5, 1865.

(Abstract.)

The amount of a chemical change under any conditions which allow of its completion, depends ultimately upon the amount of that one of the substances partaking in it which is present in the smallest proportional quantity. But if the change be arrested before any one of the reagents is exhausted, its amount depends upon the conditions under which it has occurred. These conditions, in the simplest cases, are the quantity of the several reagents, their temperature, and the time during which they have been in contact. The laws of connexion between these conditions of a chemical change and its amount are the subject of an investigation upon which the authors have entered. An account of the first stage of this investigation is contained in the present paper.

Although every chemical change is undoubtedly governed by certain general laws relating to the conditions under which it occurs, the number of cases in which the investigation of these laws is possible is extremely limited. For it is requisite both that the amount of change should be readily estimated, and also that all the conditions affecting it should be susceptible of measurement and of such independent variations as must be made in order to determine the separate influence of each.

The first reaction chosen for investigation was that of permanganic acid upon oxalic acid. It is well known that when a solution of potassic permanganate is added to a solution containing an excess of oxalic acid and sulphuric acid, a change takes place which in its final result is represented by the following equation:—

$$K_2Mn_2O_8+3H_2SO_4+5H_2C_2O_4=K_2SO_4+2MnSO_4+10CO_2+8H_2O.$$

This reaction occurs at the ordinary temperature; it is thus comparatively easy to keep the temperature of the solution absolutely constant during its progress. It occupies, under a due arrangement of other conditions, a convenient interval of time, and can be started and terminated at a given moment. The reagents are readily obtained in a state of purity, and can be accurately divided and measured as liquids. Lastly, no other condition besides those named affects the result: when each of these is fixed, the amount of change observed in successive experiments is always the same. Nevertheless this reaction, as appeared in the course of its investigation, is

not well adapted to the purpose in view. It is not chemically simple. More than one change occurs under the circumstances of the experiment, and the equation above written represents only a net result. But the examination of a second and simpler reaction in which the authors are at present engaged, has confirmed an explanation which had already suggested itself to them of the results of this series of experiments, and thus they are now enabled to present these results together with a theory which explains and is supported by them.

The effect of varying the amount of each of the reagents and the duration of the action was successively investigated. The remaining condition of temperature was not made the subject of experiment, owing to the discovery of the complex nature of the chemical change. A series of Tables contain the numerical results of these experiments. The principal complication arises from a secondary reaction which takes place between permanganic acid and the manganous salt formed by its reduction. It became necessary, in consequence of this action, to include manganous sulphate among the reagents the effect of whose variation was to be determined.

The general method of experimenting was briefly as follows:—Measured quantities of the standard solutions of oxalic acid, sulphuric acid, and manganous sulphate were mixed with a measured quantity of water and the whole brought to a temperature of 16° C. A measured quantity of a standard solution of potassic permanganate was added, and the time of the addition noted. Throughout the course of the action the temperature, observed by means of a thermometer passing into the fluid, was kept rigorously constant. When the required interval had elapsed, an excess of potassic iodide was thrown in, and the liberated iodine, which furnishes an exact measure of the residual permanganic acid or manganic oxide, estimated by means of a standard solution of sodic hyposulphite. The amount of chemical change occurring in any given time with any given amounts of the several reagents can thus be determined.

1. *Variation of Sulphuric Acid.*—Each experiment of this series was allowed to proceed for four minutes. Oxalic acid and potassic permanganate were employed in the proportions in which they act one upon another. The quantity of sulphuric acid was varied from the proportional quantity up to seven times that amount. A regular increase in the amount of chemical change within the allotted time occurs with each increment of sulphuric acid. The relation of these quantities, which formed the subject of many series of experiments, is, however, of a complex character. Two or three reactions, it is shown, occur simultaneously, and each of these is influenced by the acidity of the solution.

2. *Variation of Manganous Sulphate.*—At the ordinary temperature in a dilute and feebly acid solution, permanganic acid acts very slowly upon oxalic acid, but the presence of a manganous salt, formed by its reduction or previously added, causes a great acceleration. This acceleration is shown to reach a maximum when three molecules of manganous sulphate are taken to one of permanganate. By the reaction of these quantities man-

ganic binoxide is formed according to the equation

$$K_2Mn_2O_8+3MnSO_4+2H_2O=K_2SO_4+2H_2SO_4+5MnO_2.$$

3. *Variation of Oxalic Acid.*—The results obtained in this series of experiments are at first sight paradoxical. The quantity of permanganate reduced in three minutes, which was the time allowed to each experiment, increases with the proportion of oxalic acid up to a certain point; it then diminishes until another point is reached, after which further additions of oxalic acid produce again a very gradual acceleration. The result is the same whether only oxalic acid, potassic permanganate and manganous sulphate are taken, or whether sulphuric acid is added to these. The maximum action occurs with five molecules of oxalic acid and one of permanganate, that is with proportional quantities. The second and constant minimum is nearly attained with ten molecules of oxalic acid. Probably the manganic binoxide formed by the reaction of manganous sulphate and potassic permanganate combines with an excess of oxalic acid to form a compound whose decomposition proceeds more slowly than the action of free binoxide upon it. The conditions of the minimum action may be thus represented:—

1. $MnO_2+2H_2C_2O_4=MnC_4O_8+2H_2O.$
2. $MnC_4O_8=MnC_2O_4+2CO_2.$

There is found in the first instance a clear brown solution, the colour of which slowly fades.

4. *Variation of the Time.*—If it were possible for all other conditions of a chemical change to remain constant, if, for example, the substances reacting could be added in proportion as they disappeared, and those formed either were without influence or could be removed, the effect of a variation of time might be confidently predicted. In such a case the total amount of chemical change would be directly proportional to the duration of the action. But when one or more of the substances diminishes in quantity as the change proceeds, the relation is no longer of this simple character. Experiments upon this relation form the remaining and chief part of this inquiry. Each experiment of a series exactly resembled every other except in the time allowed to elapse before the action was interrupted. And thus each series may be regarded as exhibiting the course of a single experiment, and showing how much of the active substances still remain at any time from its commencement.

In the earlier series the reagents were employed in proportional quantities, and it was observed that for most of the determinations the product of the number expressing the duration of the action and of the number expressing the amount of active substance still remaining, is a constant quantity. The first stages of the action exhibit, however, a divergence from this law. This divergence is explained by reference to the simultaneous occurrence of two gradual actions, that in which manganic binoxide is formed and that in which it is reduced. The inverse proportionality of the residue to the duration of the action when two substances present in

proportional quantities are destroying one another, is shown to follow from a law the generality of which the authors hope to establish—namely, that the total amount of chemical change varies directly with the amount of each of the substances partaking in it.

In the later series of experiments the necessary condition, that the ratio of the reagents should remain constant throughout the action, was fulfilled by taking all but one of them in great excess as compared with that one. Under these circumstances a single substance gradually disappears, all around it remaining unchanged; and according to the law above enunciated, the total amount of change occurring at any moment is proportional to the quantity of substance then remaining. It is shown that if this be the case, the numbers representing the amounts of residue after equal intervals of time should form a series in geometric progression. This relation is actually exhibited by some of the experimental series; but the greater number of them do not conform to it. The reason of this is to be found in the fact that more than one reaction occurs under the circumstances of these experiments, and that it is only possible to measure the total effect. Experiments upon the solution in which the gradual oxidation of oxalic acid has taken place are adduced to show that some other oxidized product besides carbonic acid is formed, and it is inferred that more than one agent takes part in its oxidation. Also the facility with which hydrated peroxide of manganese reacts with dilute sulphuric acid and manganous sulphate to form a solution of mangano-manganic sulphate renders it probable that this salt is produced in the experiment. With an excess of oxalic acid and manganous sulphate the red colour of potassic permanganate disappears as soon as this salt is added to the mixture. The formation of manganic binoxide appears to be instantaneous. It finds itself in presence of two substances, both of which act gradually upon it—oxalic acid and manganous sulphate, the latter producing an intermediate oxide, probably the protosesquioxide, which is also reducible by oxalic acid. It is possible that other oxides besides these may be formed; but it is almost certain, from the experimental results, that the action is not more simple than this. At the end of each experiment both or all of these oxides are alike instantaneously reduced by hydriodic acid and thus measured conjointly. Finally it is shown that an equation may be constructed embodying this hypothesis, and that all the series of experimental numbers may be expressed by equations of this form. The paper concludes with a mathematical discussion, by Mr. Esson, of various points in the theory of this action. An outline of his statement is here appended.

When a single substance is undergoing chemical transformation under constant conditions, it is shown by experiment that the law of connexion between y, the quantity of substance remaining unchanged, and x, the time during which the change has been proceeding, is $y=a\alpha^x$; where a is the quantity of substance present at the beginning of the change, and α a con-

stant which depends upon the conditions under which the change takes place. From this equation is derived $dy \propto y dx$, which expresses the fact that the amount of change varies directly with the time and with the quantity of substance.

Cases of complex chemical change can be investigated by the application of this general law. When two substances are reacting in proportional quantities, the amount of change is proportional to the amount of each, and the equation for determining the character of the reaction is $dy \propto y^2 dx$, or $\frac{1}{y} - \frac{1}{a} = \frac{x}{b}$, where a is the quantity of substance present at the beginning of the change. If a is very large, the equation reduces to $xy = b$, i. e. the quantity of substance remaining unchanged varies inversely as the time.

It is shown that the complexity of the results obtained in the experiments on the decomposition of potassic permanganate is probably due to the fact that there are two substances undergoing change, and that one of these substances is gradually formed from the other. The equations for determining the character of this reaction are

$$\left.\begin{aligned}\left(\frac{du}{dx}\right) &= -(\alpha+\beta)u,\\ \left(\frac{dv}{dx}\right) &= \quad \beta u - \gamma v.\end{aligned}\right\}$$

From these equations are derived

$$u = ae^{-(\alpha+\beta)x}, \quad \ldots \quad (1)$$

$$v = \frac{a\beta}{\alpha+\beta-\gamma}\left\{e^{-\gamma x} - e^{-(\alpha+\beta)x}\right\}, \quad \ldots \quad (2)$$

$$y = \frac{a}{\alpha+\beta-\gamma}\left\{\beta e^{-\gamma x} - (\alpha-\gamma)e^{-(\alpha+\beta)x}\right\}, \quad \ldots \quad (3)$$

where u is the quantity of one substance decomposed at the rate α, v the quantity of the other substance formed from u at the rate β and decomposed at the rate γ, y the whole quantity of substance capable of change, a the quantity of substance present at the beginning of the change, and x the time during which the change has been proceeding. Equation (3) admits of the forms

$$y = a_{\prime}\alpha_{\prime}^{x} - b_{\prime}\beta_{\prime}^{x},$$
$$y = a_{\prime}\alpha_{\prime}^{x},$$
$$y = a_{\prime}\alpha_{\prime}^{x} + b_{\prime}\beta_{\prime}^{x},$$

according as α is $> = < \gamma$. By varying continuously one of the conditions of the reaction, it is possible to obtain in succession values of α and γ, such that α is first $> \gamma$, and then $= \gamma$, and finally $< \gamma$; and thus these three forms of curves may occur in an investigation on the effect of varying one of the conditions of a reaction of this kind.

Supplementary Note to Dr. Davy's Paper on Birds; received November 25, 1865.

Mention is made in the paper referred to of the comparatively low temperature of certain birds. Another example of the same kind occurs in the goose; in two instances I have found its temperature *in recto* 104°, and in a third 103°·5. The trials were made in November; the geese had not previously been confined, were about seven months old, fully feathered (few birds have a warmer clothing), and the weather at the time was moderate; the temperature of the open air between 40° and 50° Fahr.

Notice is also taken of a bird, the grouse, not remarkable for power of flight, having air in its femora as well as in its humeri. I have since found another example of the same kind in the pheasant, a bird even of feebler flight; in no instance, and I have examined several specimens, have I detected marrow in either of these bones.

In reference to the statement implying that those bones of birds which contain air in their adult state, in an earlier stage contain marrow, later observations have led me to infer that, instead of marrow, these bones have their canals impacted with blood-vessels, which in process of the bird's growth shrink and are absorbed.

November 23, 1865.

Lieut.-General SABINE, President, in the Chair.

In compliance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Council and Officers proposed for election was read as follows:—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L.

Foreign Secretary.—Professor William Hallows Miller, M.A.

Other Members of the Council.—John Frederic Bateman, Esq.; Lionel Smith Beale, Esq., M.B.; William Bowman, Esq.; Commander F. J. Owen Evans, R.N.; Edward Frankland, Esq., Ph.D.; Francis Galton, Esq.; John Peter Gassiot, Esq.; John Edward Gray, Esq., Ph.D.; Thomas Archer Hirst, Esq., Ph.D.; Sir Henry Holland, Bart., M.D., D.C.L.; William Odling, Esq., M.B.; Sir John Rennie, Knt.; Prof. Warington W. Smyth; William Spottiswoode, Esq., M.A.; Paul E. Count de Strzlecki, C.B., D.C.L.; Vice-Chancellor Sir W. P. Wood, D.C.L.

Dr. Robert M'Donnell was admitted into the Society.

Pursuant to notice given at the last Meeting, The Right Honourable Charles Pelham Villiers was proposed for immediate ballot.

The proposal having been seconded, the ballot was taken, and Mr Villiers was declared duly elected a Fellow of the Society.

Mr. Villiers was afterwards admitted into the Society.

The following communications were read :—

I. "On Calorescence." By JOHN TYNDALL, F.R.S. Received October 20, 1865.

(Abstract.)

The paper is divided into ten short sections. In the 1st the experiments of Sir William Herschel and of Prof. Müller on the sun's radiation are described. In the 2nd are given a series of measurements which show the distribution of heat in the spectrum of the electric light. In the 3rd section is described a mode of filtering the composite radiation of an intensely luminous source so as to detach the luminous from the non-luminous portion of the emission. The ratio of the visible to the invisible radiation determined in this way is compared and found coincident with the results of prismatic analysis. The eminent fitness of a combination of iodine and bisulphide of carbon as a *ray-filter* is illustrated, and in the 4th section experiments with other substances are described; various effects obtained in the earlier experiments on the invisible rays being mentioned. In the 5th section the absolutely invisible character of the radiation is established; it is also proved that no extra-violet rays are to be found at the obscure focus. Numerous experiments on combustion produced by invisible rays are also described in the 5th section. The 6th section deals with the subject of *calorescence,* or the conversion of obscure radiant heat into light. In section 7 various modes of experimenting are described by which the danger incident to the use of so inflammable a body as the bisulphide of carbon may be avoided. In the 8th section are described experiments on the invisible radiation of the lime-light and of the sun. In the 9th section the effect obtained by exposing papers of different colours at the dark focus are mentioned; while the 10th and concluding section, deals with the calorescence obtainable from rays transmitted by glasses of various kinds.

II. "Notice of the Surface of the Sun." By JOHN PHILLIPS, M.A. LL.D., F.R.S., &c., Professor of Geology in the University of Oxford. Received October 27th, 1865.

It appears desirable, as a first step to a right theory of the condition of the sun's surface, that the appearances which it presents should be recorded in some systematic way. Photographs will suffice for the distribution of the *spots,* but careful eye-drawings must be appealed to in evidence of the form, arrangement, and intestine motions of the parts of those spots, and eye-drawings with measures are the only means of recording accurately the dotted, areolar, granular, crested, and other arrangements

which under the general title of "porosity" have been recognized over the whole face of the sun. Descriptions cannot be complete, but, what is more, they may be, and probably often are, misleading—words which call up right ideas of things often fail very much when required to perform the same function for new objects not well understood. With this conviction in my mind, I have requested the Royal Society to accept a few drawings representing features on the sun's face as they appear to me looking through a telescope of known dimensions, and used in a certain way. If observers would send sketches made at the telescope, showing what they see, or think they see,—not finished paintings to illustrate hypothetical ideas,—these sketches, by gradual accumulation and comparison, would at last furnish evidence by which even a great theory might be brought to a satisfactory test. Therefore it is that I presume to offer to the Royal Society some additional sketches of the "porosity" of the sun, as seen in good observing weather in this month of October*.

The whole surface of the sun, as seen on the 24th and 25th, appeared quite free from any dark patches large enough to be called spots—offering in this respect a singular contrast with its aspect on the 17th, when the large doubly nucleated spot, of which I sent a sketch a few days since, was so conspicuous near the (apparent) right edge†.

On this apparently even and marble-like surface, a power of 50, with the full aperture of 6 inches, made manifest the existence of the porosity at every point, from the centre to near the edge, the distinctness being greatest over all the middle part of the area. By applying successively powers of 75, 100, 135, and 180, it was easy to observe the general effect, and the particular features of diversity. The clock-rate being regulated exactly, any particular part of the disk might be kept continually under view; and to increase the distinctness of the object, or rather the comfort of the observer in looking at it, the field was contracted by diaphragms to one-half or one-third of the usual diameter. The great obstacle to a strict observation of any small selected part of the sun's disk is unsteadiness of the head, a circumstance troublesome to portrait-photographers, but more injurious to astronomers. I believe this kind of error to be one of the elements of personal equation, and that it can sometimes only be cured by allowing the observer to take hold of the moving telescope. This, thanks to Mr. Cooke's solid construction, can be safely done.

The sketches now presented relate only to the appearances presented to one observer, with the precautions stated; to what degree they are affected by "personal equation" remains to be proved by comparison with others, and I hope better drawings.

* Drawings made on a former occasion, and presented to the Royal Society, may be referred to for comparison. (See Plate XII. fig. 6.)

† Telescope furnished with diagonal glass, the rays reflected to the West. By this arrangement the usual reversal of the object in every direction becomes limited to the vertical direction.

Pl. XII. fig. 1 represents a part of the surface under a low power (75), which is carefully moved out of focus inwards and outwards. Under these conditions, the soft undefined mottling which it shows catches the eye, and appears clearly to be caused by parts not differing in structure from the more shadowy spaces between them, except by there being less effect of shadow points and lines on the parts which are relatively lighter. Here and there apparently dark specks appear, either in the darker tracts or on the lighter parts; and there are specks of all degrees of darkness, as well as lines of greater or less distinctness.

Pl. XII. fig. 2 is offered as a careful attempt to copy a definite tract, still employing a low power (75), and using every means to get the focus exactly. When this is accomplished, and the eye placed as close as possible to the eyepiece, the appearances can be sketched as well as an artist can picture a tree with its leaves, a heap of broken stones, or some dissected and areolated clouds. They can be sketched, but certainly not well or truly, without patient attention, and eyes and head in a good state. Here the texture appears to be areolar, with much irregularity in the shapes, but no great inequality of size. Dots of extremely small dimensions, sometimes quite black, appear singly or in pairs in the centres of several areolæ.

Pl. XII. fig. 3. Another sketch, under the same conditions, but employing powers of 135 and (rarely) 180. In this part of the disk dots, occasionally running together into a complicated short tract, may be seen, not specially conformed to the areolar structure, but in some places crossing it, and elsewhere scattered about it. The number of short irregular discontinuous lines which occur mixed with dots is very great; none of them appear to be regularly curved or regularly straight, but seem to be intervals merely between more enlightened parts. It does not seem to me that dots of greater darkness *usually* appear at the intersections or terminations of these fissure-like objects.

Pl. XII. fig. 4, is intended to convey the impression arising from a close study of one small space quite definite in character and marked by distinct small dots, one elongated in the middle part of a subpentagonal space, around which other less regular areolæ were gathered. After much attention, it appeared to me that the boundaries of this rude pentagon were in part broken up into irregular short loops and dots; and though the observation was difficult, I am not afraid to trust it. This selected space is drawn on a larger scale, but it was not seen with higher powers than No. 3.

Pl. XII. fig. 5 shows a curious areola with a black central dot, and three parallel markings on the boundary.

Pl. XII. fig. 6, a sketch made in April 1864, is introduced for comparison, and especially for the softly luminous mottling of the surface.

I shall be very glad to be informed whether what is here said agrees or not with the observations of other persons, made with other instruments.

Supplementary Note, Nov. 25, 1865.

The spot to which the above notices refer has been made the subject of careful observations by M. Chacornac, who has issued interesting descriptions and drawings of it from October 7 to October 16. The Rev. Mr. Howlett has also scrutinized the same object, and prepared drawings to October 17, the day when my first sketch was made. Thus we have for this spot observations through one rotation and a half, and we may perhaps have the pleasure of welcoming it again in a new form.—J. P.

III. "Notice of a Spot on the Sun, observed at intervals during one Rotation." By John Phillips, M.A., LL.D., F.R.S., Professor of Geology in the University of Oxford. With Drawings. Received November 15, 1865.

On the 17th of October, 1865, at 2 p.m. the spot referred to had traversed a great portion of its arc, and was approaching the limb. It showed two large unequal umbræ, and in each of them a blacker nucleus. Between them were several small dark dots, partially coalescent. The edges of the umbræ were very irregular. In the smaller umbra two bright dots. Above the larger umbra (which appeared to the right in the telescope) was an exceptionally bright band, traversed by two dark threads ending in dark dots. This band crossed a part of the umbra, like a bridge, but itself was there traversed by a small bar. Four bright patches lay in the continuation of this facula toward the prolonged upper (apparently) extremity of the penumbra, which was itself more luminous than other penumbral parts. The penumbra had broken edges, and an interior mottling of small brighter and darker spaces directed variously toward the umbræ. The granulated surface of the sun with soft gleaming facular ridges was conspicuously seen, and tracts of darkly dotted surface were seen beyond each extremity and on one side (Pl. X. fig. 1).

Nov. 4, 9.45.—The spot had now returned by rotation, and was very distinctly seen amidst far extended clouds of bright faculæ, though reduced to less than half its former dimensions. It retained the two umbral tracts; but it was now the left-hand tract which was the larger. Being about 15° from the limb, the general figure was oval, as usual; the umbræ were of irregular figure, the larger one cut into by bright branches from the interumbral space. Dark dots amidst the faculæ on the border (Pl. X. fig. 2).

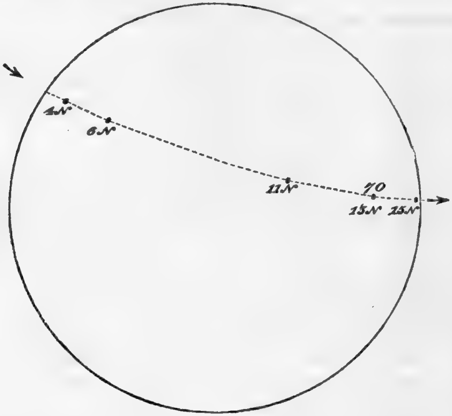
Nov. 6, 9.45.—The spot had reached about 45° from the edge, and appeared less elliptical, and otherwise changed in aspect. The large umbra was much dissected by bright streams, and the smaller one had assumed a distinctly tripartite shape. The edges of the penumbra appeared rugged. Many small spots and dark dots towards the edge of the disk (Pl. X. fig. 3)

Nov. 11, 9.45.—The spot had now passed the central meridian, and was greatly altered, and almost cut into two parts by a bright facular mass, passing between the umbræ. The larger of these is now in a pentagonal

form, and has a bright central speck, with a rather obscure narrow prolongation. The smaller umbra is tripartite, and has small gleaming points in it. Several black dots in the surrounding surface, amidst faculæ, areolæ, and other structures very distinctly seen for a great part of this day with good definition (Pl. XI. fig. 4).

Nov. 13, 9.45.—Very great change in general figure and in the several parts of the spot. The larger spot is now cut in twain; the smaller spot is reduced to two dots, surrounded by a large bright space. The spot is now about 36° from the limb (Pl. XI. fig. 5).

Nov. 15.—The spot is very near the edge, and of course almost elliptical in outline (Pl. XI. fig. 6). The faculæ which accompany it are seen on a smaller scale in Pl. XI. fig. 7.



The woodcut shows the apparent places of the spot at the several dates mentioned.

EXPLANATION OF THE FIGURES IN PLATES X., XI., XII.

Pl. X. fig. 1. Appearance of spot near the edge of the disk before passing off.
2. Spot after reappearance, within the opposite edge of the disk.
3. The same farther on the disk.
Pl. XI. fig. 4. The same after passing the centre of the disk.
5. The same advancing toward the edge.
6. The same very near the edge.
7. The same surrounded by faculæ. This figure is drawn on a smaller scale than the others.
Pl. XII. figs. 1, 2, 3, 4, 5. Sketches taken in October 1865.
6. Sketched in April 1864.

November 30, 1865.

ANNIVERSARY MEETING.

Lieut.-General SABINE, President, in the Chair.

Mr. Bowman, on the part of the Auditors of the Treasurer's Accounts appointed by the Society, reported that the total receipts during the last year, including a balance of £683 14*s.* 1*d.* carried from the preceding year, amounted to £4882 0*s.* 1*d.*; and that the total expenditure in the same period, including £905 invested in the Funds, amounted to £5001 18*s.* 11*d.*, leaving a balance of £135 7*s.* 10*d.* due to the Bankers, and £15 9*s.* i the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

Royal.

His Imperial and Royal Highness the Archduke Maximilian of Austria.

On the Home List.

John George Appold, Esq.
George Boole, Esq.
George William Frederick Howard, Earl of Carlisle, K.G.
Samuel Hunter Christie, M.A.
Joseph Dickinson, M.D.
Hugh Falconer, M.A., M.D.
Rear-Admiral Robert FitzRoy.
Renjamin Gompertz, Esq.
Richard Dugard Grainger, Esq.
Woronzow Greig, Esq.
Sir Benjamin Heywood, Bart.
Sir William Jackson Hooker, K.H., LL.D.
Rev. George Hunt, M.A.
William Thomas Horner Fox Strangways, Earl of Ilchester, M.A.
John Lindley, Esq., Ph.D.
Sir John W. Lubbock, Bart., M.A.
James Heywood Markland, D.C.L.
Sir John Maxwell, Bart.
James B. Neilson, Esq.
Algernon Percy, Duke of Northumberland, K.G.
Benjamin Oliveira, Esq.
Henry John Temple, Viscount Palmerston, K.G.
Thomas Joseph Pettigrew, Esq.
Sir John Richardson, C.B.
Sir Robert Schomburgk.
Robert William Sievier, Esq.
Admiral William Henry Smyth, K.S.F.
Henry Herbert Southey, M.D.
Rev. Robert Walker, M.A.
Thomas Williams, M.D.

On the Foreign List.

Johann Franz Encke.
Adolph Theodor Kupffer.

Withdrawn.

William Hopkins, M.A., LL.D.

Defaulter.

William Bird Herapath, M.D.

Fellows elected since the last Anniversary.

On the Home List.

Dufferin and Claneboye, Frederick Temple Blackwood, Lord, K.P., K.C.B.
Donoughmore, Richard John Hely Hutchinson, Earl of.
Turner, The Right Hon. Sir George James, Lord Justice.
H.R.H. Louis Philippe d'Orléans, Count of Paris.
The Hon. James Cockle, M.A.
Rev. William Rutter Dawes.
Archibald Geikie, Esq.
George Gore, Esq.
Robert Grant, Esq., M.A.
George Robert Gray, Esq.
George Harley, M.D.
Fleeming Jenkin, Esq.
William Huggins, Esq.
Sir F. Leopold M'Clintock, Commodore.
Robert M'Donnell, M.D.
William Kitchen Parker, Esq.
Alfred Tennyson, Esq., D.C.L.
George Henry Kendrick Thwaites, Esq.
Lieut.-Col. James Thomas Walker, R.E.
The Right Hon. Charles Pelham Villiers.

The President then addressed the Society as follows:—

Gentlemen,

In my last year's Address I informed you of the steps which had been taken, with the approval of the Council, to obtain the concurrence of Her Majesty's Government in the printing and publication of the Catalogue of the Titles of the Scientific Memoirs contained in Scientific Periodicals in all languages, from the commencement of the present century to the end of 1863, the manuscript of which had been prepared under the direction and superintendence of the President and Council, and the cost defrayed from the funds of the Society. Her Majesty's Government having been pleased to accede to the proposition that had been then made to them, the *Serial* Catalogue, as originally proposed for the Society's Library, is now in progress of rearrangement in alphabetical order according to author's names, to be followed by an alphabetical Index according to subjects. The preliminary questions regarding the type, and the form and size of the pages, having been discussed and agreed upon with the authorities of the Stationery Office, the first portion of the manuscript, containing the titles of all memoirs having the letter A as the first letter of the author's name, has been prepared, and is now placed in the printer's hands, so that the printing may be forthwith commenced.

In the meantime the endeavours to render the work more complete have not been relaxed; the number of titles, which in my last year's Address was stated to exceed 180,000, has been since extended to 213,000; and will continue to be augmented whilst the printing is in progress. It is proposed that all titles which should not be in time to be entered under their respective alphabetical heads shall be included in a supplementary

volume, which shall also comprise the titles of memoirs which must be regarded as Anonymous, having been published without the author's name. The original Serial Catalogue prepared in manuscript for the use of the Fellows of the Society still remains in the Library; and it is with great satisfaction that I am able to add that the Library itself already possesses the Transactions, Journals, and other periodical works in which two-thirds of the 213,000 titles already collected for the Catalogue are contained; and that every endeavour is making to render the Library as complete as possible in this important branch of scientific literature.

The total expenses hitherto incurred in the preparation of the Catalogue amount to £1626; and to this a small annual addition will be required until the printing and publication shall have been completed.

The Fellows of the Royal Society, and those especially who are interested in the progress of Sidereal Astronomy, will hear with pleasure that the communications, passing through Her Majesty's principal Secretary of State for the Colonies, between Sir Henry Barkly, K.C.B., F.R.S., Governor of the Colony of Victoria, and the President and Council of the Royal Society, regarding the establishment at Melbourne of a telescope of great optical power for the observation of the Nebulæ and multiple stars of the southern hemisphere, have led to a vote which has passed the Legislature of Victoria of £5000 for a suitable telescope, to be constructed under the superintendence of the President and Council of the Royal Society. In the Anniversary Address in 1862, and again in that of 1863, I availed myself of the opportunities then afforded of making known to the Fellows the progress of the communications which at each of those dates had taken place between the Government of Victoria, the authorities of the Melbourne Observatory, and the Royal Society; and I have now the satisfaction of laying before you the following letter, received on the 23rd of last month from Professor Wilson, Honorary Secretary of the Board of Visitors of the Melbourne Observatory:—

"The University, Melbourne, Aug. 21, 1865.

"My dear Sir,

"It is with very great satisfaction that I forward to you the following resolutions of the Board of Visitors adopted on the 15th inst.:—

"1. That the President of the Royal Society of London be informed that the Legislature of Victoria has voted the sum of £5000 for the purchase of an equatorial telescope, one half of which sum has been already remitted to the Crown Agents in England, and placed at the disposal of Major Pasley, of the Royal Engineers, for the purpose; and that the Government has placed the correspondence connected with it in the hands of the Board of Visitors.

"2. That the President and Council of the Royal Society be requested to give the Board of Visitors the benefit of their assistance in selecting a maker, settling the contract, and superintending the construction of the

telescope, so as best to carry out the recommendations contained in the Report of the Royal Society to the Duke of Newcastle, 18th December 1862.

"3. That Major Pasley be requested to place himself in communication with the President and Council of the Royal Society, and, after ascertaining their views, to enter into such contract as will most effectually carry them out.

"I enclose also a copy of a letter received from the Treasury, on which the foregoing resolutions are based, and a copy of the letter which I send to Major Pasley by this mail.

"The great interest which you have shown in this matter leads the Board of Visitors to count confidently on your further assistance in bringing it to a successful conclusion. The request contained in the second resolution is not intended to imply that in the opinion of the Board any further discussion as to the form of telescope or the maker is necessary. The Board thinks, and I believe that it is also your opinion, that the discussion which has already taken place has settled that question, and that Mr. Grubb's proposal should be adopted. This is not distinctly expressed in the resolution, because Mr. Grubb's name is not mentioned in the Report of the Royal Society, and because the Board desires to leave you free in the event of anything having happened to Mr. Grubb, or of any discovery having been made which would tend to modify your opinion.

"In any case the Board, bearing in mind the great length of time that has elapsed since the proposal for a telescope was first made, and having now received authority from the Government to act in the matter, is desirous of securing the completion of the telescope at the earliest possible time consistent with the highest attainable perfection in the instrument; and considers that this end will be most effectually secured by leaving you quite free to act in the matter, and trusting to you to secure the cooperation of those eminent practical astronomers whose names you mentioned as willing to superintend the work during its execution.

"Mr. Grubb's last estimate is £4600 for the telescope complete; and this, I believe, covers everything, including the erection in Ireland for a trial.

"The sum voted is £5000, and the balance, £400, will be available for a spectroscope and for a photographic apparatus adapted to the telescope, and will still probably leave sufficient to pay the freight to Melbourne. As these two adjuncts will not occupy long in making, it will probably be desirable not to commence them till the telescope proper is approaching completion, so that the latest improvements may be introduced into them.

"Trusting to your earnestness to induce you to undertake the great amount of trouble we are imposing upon you,

"I remain, my dear Sir,

"Very faithfully yours,

"W. P. Wilson,

"*Hon. Sec. to the Board of Visitors.*"

"*Major-General Sabine, R.A.,*
Pres. R.S."

To the information contained in this letter I have now the satisfaction of being able to add that since its receipt Mr. Grubb has signified his readiness to proceed in the construction of a telescope corresponding to the specification contained in his letter to Dr. Robinson of Dec. 3, 1863, printed in the second portion of the correspondence respecting the Southern Telescope,—the execution to be under the supervision of the Earl of Rosse, Dr. Robinson, and Mr. Warren De la Rue, who, on their parts, have accepted the responsibilities of superintendence. The contract between the Crown Agent for Victoria and Mr. Grubb is in process of execution, and in eighteen months from its date we may hope that the telescope will be in readiness to be embarked for Melbourne, where in the meantime preparations will be made for its reception and mounting. The selection of an Astronomer fitted by education and acquirements to be entrusted with its use at Melbourne, and who may be willing to devote his entire energies to the cultivation of the splendid field which will be open to him, must be the next anxious and important duty devolving upon the Melbourne authorities. If in its execution they should require any assistance from the Royal Society, such assistance will assuredly be most readily given.

The arrangements connected with this subject being so far advanced, I have thought it desirable to place on record a consecutive statement of the steps by which they have been brought to their present stage; and I have done this in the form of a Note (Note A)*, to avoid trespassing unnecessarily upon your present attention.

The welcome intelligence has been received from Colonel Walker, F.R.S., Superintendent of the Trigonometrical Survey of India, of the safe arrival in that country of the Pendulums prepared for the experiments which it is proposed to make at the principal stations of the survey, and of the vacuum-apparatus in which the pendulums are to be vibrated. As it has been proposed to make the Kew Observatory the Base Station of the important observations which may be made with these instruments in many parts of India, a full and very careful series of Base observations were made with them at Kew before their departure for India. These have been printed in the Proceedings of the Royal Society in the present year in the form of a communication from Messrs. Balfour Stewart and Loewy.

In the course of the last Session an important paper was communicated to the Society, and has been since printed in the Philosophical Transactions for 1865, Art. V., entitled "On the Magnetic Character of the Armour-plated Ships of the Royal Navy, and on the Effect on the Compass of particular arrangements of Iron in a Ship," by Staff-Commander Frederick John Evans, R.N., F.R.S., Superintendent of the Compass Department of Her Majesty's Navy, and Archibald Smith, Esq., F.R.S.

In the course of the reading of this paper, and in the discussion which

* See note A, p. 503.

followed it, the absence of any proper provision for the instruction or guidance of the builders, fitters, and navigators of the ships of our mercantile marine was strongly dwelt upon. It is well known that the number of iron ships recently constructed greatly exceeds that of wood-built ships. In such vessels iron is now used, not only in the construction of the hull, but in decks, deck-houses, masts, rigging, and many other parts of the ship, for which wood was till recently used. The consequence has been a great increase in the amount of the deviation of the compass, increased difficulty in finding a suitable place for the compass, and an increased necessity for, and difficulty in, applying to the deviation either mechanical or tabular corrections.

Many recent losses of iron steamers have taken place, in which there is reason to believe that compass-error occasioned the loss. In most of these, however, from the want of any record of the magnetic state of the ship, of the amount of the original deviation, and of the mode of correction—and from the investigations into the causes of the loss having been conducted by persons uninformed or not interested in the science, and who are necessarily incompetent therefore either to elicit the facts from which a judgment can be formed, or to form a judgment on those facts which are elicited—no certain conclusion as to the cause of the loss can be arrived at. The investigations are, however, sufficient to show the want of a better and more uniform system of compass-correction in the mercantile marine, and of more knowledge of the subject on the part of those who are entrusted with the fitting and navigation of these ships.

Acting in conformity with the opinions expressed in the discussion which followed the reading of the paper by Commander Evans and Mr. Smith, and availing themselves of the counsel of those who are justly regarded as possessing the greatest practical experience on such subjects in this or any other country, the President and Council addressed a letter to the President of the Board of Trade, bringing under his consideration a subject which they have reason to believe is of pressing importance, requiring that measures of a more stringent and effective character should be taken in the direction already followed by Her Majesty's Government in such legislative enactments as those contained in the Merchant Shipping Act of 1854; and, impelled by a strong conviction of the impending danger, they have ventured to suggest the expediency of steps being taken for the mercantile marine similar in character to those which have been found to work so successfully in the Compass Department of the Royal Navy.

The lamented decease of the late Admiral FitzRoy induced a desire on the part of the Board of Trade to review the past proceedings and present state of the department of that Board which had been placed under Admiral FitzRoy's direction. Adverting to the fact that at the formation of that Department the Board of Trade had requested the opinion of the Royal Society as to what might then be considered the great

desiderata in Meteorological Science, and had received in reply a letter from the President and Council (dated February 22, 1855) containing recommendations which were eventually adopted as the basis of the proceedings of the Meteorological Department of the Board of Trade, the Board was now desirous of being informed to what extent those objects had been fulfilled by what had already been accomplished, and whether the objects which had been so specified were still considered as important for the interests of science and navigation as they were then considered.

The Board of Trade were also desirous of obtaining an opinion from the Royal Society regarding the Forecasts of Weather and the Storm Warnings which had not been included in the original recommendations of the Royal Society, but had originated with Admiral FitzRoy himself and had formed a considerable part of the duties of the Meteorological Department since 1859.

To enable the President and Council to form a judgment on the questions referred to them, the Board of Trade supplied them with the following documents :—

1. Admiral FitzRoy's Report to the Board of Trade, dated May 1862.

2. A Report by Mr. Babington (Admiral FitzRoy's first assistant) on the method adopted in the department with regard to forecasts and storm-warnings.

3. A return to the House of Commons, dated April 13, 1864, presenting a comparison of the probable force of the wind as indicated by the signals in the year from April 1, 1863, to March 31, 1864, and its actual state as reported in the three days following the exhibition of the signals.

4. A manuscript return, furnished by Mr. Babington, having the same object for the year from April 1, 1864, to March 31, 1865.

The first of these documents contained the opinions of the Shipmasters at several ports on our coasts, officially requested and given, in regard to the practical value which they attached to the storm-warnings. Of these replies, by far the greater number were decidedly favourable, three only out of fifty-six being decidedly unfavourable. The date of the Report containing them was May 1862; and the two subsequent Reports, dated respectively in 1864 and 1865, exhibited in comparison a marked improvement in successive years. Upon the authority of those statements, and viewing the system of forecasting which Admiral FitzRoy had instituted simply (as described by himself) as "an experimental process," based on the knowledge conveyed by Telegraph of the actual state of the winds and weather and other meteorological phenomena within a specified area, and on a comparison of these with the telegrams of the preceding days, so as to obtain inferences as to the probable changes in the succeeding days—taking into account also the evidence supplied of the improvement in the forecasts of each year compared with those of the preceding year—the President and Council were of opinion that it was not unreasonable to anticipate that the system, so far at least as regarded the storm-warnings, if

continued, might receive still further improvement; and that possibly the best arrangement at the present time would be that this branch of the duties of the office should continue as at present, and be carried on under the direction of Mr. Babington, by whom it had been virtually superintended for several months past.

With reference to those branches of inquiry which had been originally suggested by the Royal Society in the letter of the President and Council of February 22nd, 1855, the reply, as might reasonably be expected, was of a more decided character. The most prominent amongst the objects recommended in that letter was the collection and coordination of facts bearing on what may perhaps not improperly be termed Oceanic Statistics,—viz. all such facts as are required to enable a correct knowledge to be formed of Currents of the Ocean, their direction, extent, velocity, and the temperature of the water relatively to the ordinary ocean temperature in the same latitude, together with the variations in all these respects which currents experience in different parts of the year and in different parts of their course. These, as well as the facts connected with the great persistent barometric elevations and depressions which we know to exist in several oceanic localities, leading to a knowledge of their causes, as well as of their influence on circumstances affecting navigation, were noticed in the letter of February 1855 as inquiries well deserving the attention of a country possessing such extensive maritime facilities and interests as ours, and as likely to form a suitable contribution on our part to the general system of meteorological inquiry which had then recently been adopted by the principal continental states in Europe and America.

It was learnt from Mr. Babington that much had been done by Admiral FitzRoy in the three or four years succeeding the establishment of his office (and before the subject of storm-warnings had engrossed the greater part of his thoughts), in directing the attention of many of the commanders of our merchant ships to the collection of suitable data, and in improving their habits of observation and of record. The logs of such vessels, we were informed, constitute at present a large collection of documents existing in the office of the Board of Trade, partially examined, and their contents partially classified. A full and careful examination of these for the purpose of ascertaining the amount and value of their contents was our first recommendation, to be combined with a consideration of the most fitting mode in which the information they might be found to contain may be made available for public use. Such an examination may also be expected to lead to improvements in the instructions which have been issued to our merchant seamen, who have doubtless become more competent to conduct, and even to extend, the observations for these and similar purposes, than when the system was first introduced. Those amongst us who have read with the attention it deserves the admirable paper in which Captain Henry Toynbee has enriched our Proceedings in the past year with the results of his five Indian voyages, will not doubt the

competency or the disposition that may exist amongst our merchant seamen to collect materials of the highest value for the investigations which the President and Council originally recommended; and we can entertain no doubt that, whatever may prove to be the amount and value of the materials already collected, they will form but a small contribution towards that general embodiment of the statistics of the ocean which the great increase of our commercial activity makes of pressing importance, and which may be expected to shorten materially the passages between distant ports.

The Board of Trade were also desirous to know whether the Royal Society has any recommendations to make with reference to what may be called "Meteorology proper," viz., meteorological observations to be made *on land*, in addition to the marine observations which were so strongly urged in the letter of the President and Council of February 1855.

The reason why the advantages to be derived from a well-directed system of *maritime* observations was more particularly pressed on that occasion was, that neither the instruments nor the modes of observation suitable for a well-organized, general, and efficient system of land meteorology had been then prepared. The circumstances which constituted the difficulty in this respect were well stated by Lieut. Maury in a letter addressed to the United States Government, dated November 6, 1852, subsequently transmitted by the American minister to the Earl of Clarendon, and printed in the papers preceding the Brussels conference, which were presented to the House of Lords in February 1853. This difficulty no longer exists, having been wholly obviated by the self-recording system of observation, for which the necessary instruments have been devised and brought into use at the Kew Observatory.

The President and Council have had therefore no hesitation in expressing the opinion that systematic meteorological observations at a few selected land stations in the British Islands are desirable, in addition to the marine meteorological observations, in order to complete a suitable contribution from this country to the meteorological observations now in progress in the principal states of Europe and America, under the authority of their respective Governments. A few stations, say six, distributed at nearly equal distances in a meridional direction from the south of England to the north of Scotland, furnished with *self-recording* instruments supplied from and duly verified at one of the stations regarded as a central station, and exhibiting a *continuous* record of the temperature, pressure, electric and hygrometric state of the atmosphere, and the direction and force of the wind, might perhaps be sufficient to supply an authoritative knowledge of those peculiarities in the meteorology of our country which would be viewed as of the most importance to other countries, and would at the same time form authentic points of reference for the use of our own meteorologists. The scientific progress of meteorology from this time forward requires indeed such continuous records—first, for the sake of the knowledge which they alone can effectively supply, and next for the comparison with the

results of independent observation not continuous. The actual photograms, or other mechanical representations, transmitted periodically by post to the central station might be made to constitute a lithographed page for each day in the year, comprehending the phenomena at all the six stations—each separate curve admitting of exact measurement from its own base-line, the precise value of which might in every case be specified.

The President and Council have added a suggestion that the Observatory of the British Association at Kew might, with much propriety and public advantage, be adopted as the central meteorological station. It already possesses the principal self-recording instruments, and the greater part of these have been in constant use there for many months. There would be no difficulty in obtaining similar instruments for the affiliated meteorological stations, and in arranging for their verification and comparison with the Kew standards, as well as in giving to those into whose hands they may be placed, such instructions as may ensure uniformity of operation.

You are aware that Royal Princes, Foreign as well as British, who signify their desire to enter the Society, and are proposed accordingly, are understood to be entitled to immediate ballot. On a late occasion, however, it was found that, according to the strict letter of the statutes, the head and representative of a Royal House might be inadmissible by privileged election, whilst members of the same family of inferior rank were entitled to it. His Royal Highness the Count of Paris having expressed a desire to join our body, it appeared on referring to the Statutes, that although he is the son of the late Duke of Orleans and hereditary representative of the late King of the French, yet, inasmuch as his father had not been a "sovereign prince," the Society was precluded from showing him a courtesy which it may extend to other members of his family who look up to him as the head of their house. The Council, believing that the Society would desire to see this anomaly corrected, took, after due deliberation, the prescribed steps for amending the Statute; and being advised that the usage of Her Majesty's Court would afford a suitable criterion of rank applicable to the case, introduced words extending the privilege in question to "any foreign Prince who is received by Her Majesty as Imperial Highness or Royal Highness." The unanimous election of the Count of Paris under the amended Statute may, I think, be taken as a ratification of the act of the Council.

I am glad to avail myself of this opportunity of stating that the reduction of the automatic records of the bifilar magnetometer at Kew during the seven years from 1858 to 1864 inclusive has now been completed, so far as to make known the relative amount of magnetic disturbance in each of those years. The results are shown in a note (B)*, by which it will be seen that 1859 was a year of decided maximum, the aggregate disturbances in that year being considerably greater than in 1858, and dimi-

* See note B, p. 512.

nishing progressively from 1859 to 1863–1864: in 1863 and 1864 the amount of disturbance was nearly identical, and was only about one-third of the amount in 1859. From the general aspect of the photographic traces in the present year (1865), there appears reason to believe that the epoch of minimum is now passed. If this be so, the years 1863–64 will have been the fourth return of the epoch of minimum since 1823-24 (Arago's Meteorological Observations, English translation, Editor's Note, pages 355 to 357), thus confirming the coincidence with the *decennial* variation of the sun-spots discovered by Schwabe.

Those who regard with interest the progressive establishment of the theory which assigns a cosmical origin to the Terrestrial Magnetic Variations, will have noticed the remarkable, but not altogether unanticipated, testimony borne to the decennial variation by the annual values of the magnetic Inclination at Toronto in the years from 1853 to 1864, in the volume recently published by Mr. Kingston, Superintendent of that Observatory. The general effect of the disturbances of the Inclination at Toronto is to increase what would otherwise be the amount of that element; therefore, if the disturbances have a decennial period, the absolute values of the Inclination (if observed with sufficient delicacy) ought to show in their annual means a corresponding decennial variation, of which the minimum should coincide with the year of minimum disturbance, and the maximum with the year of maximum disturbance. I have placed in a note (C)* the annual values derived in each case from the regular monthly determinations, commencing with 1853, and ending with 1864, taken from the publication referred to, whereby it will be seen that an actual variation does exist such as I have indicated, 1853 being a minimum and 1859 a maximum; the increasing progression being uninterrupted from 1853 to 1859, and the decreasing progression uninterrupted from 1859 to 1864, the date of the latest published results.

It was in the year 1853 that the Toronto Observatory was transferred to the provincial authorities, and was placed by them under the direction of Mr. Kingston. The Inclinometer employed is the same which was described in a paper in the Philosophical Transactions for 1850, Art. IX., entitled "On the Means adopted in the British Colonial Magnetic Observatories for determining the Absolute Values, Secular Changes, and Annual Variations of the Terrestrial Magnetic Elements;" and the assistants by whom the observations were made were the same persons who had performed the same duties when the Observatory was under the direction of Officers of the Artillery. The results are a valuable exemplification of the accuracy attainable when proper attention is paid to the selection of the instruments, and to the employment of careful and skilful observers. Such evidence is of more than ordinary interest at the present time, when such institutions are rapidly increasing.

We have recently learned by a despatch from Sir H. Barkly, Governor of

* See note C, p. 513.

Mauritius, to the Secretary of State for the Colonies, a copy of which has been transmitted to the Royal Society by Mr. Cardwell, that arrangements have been made and funds provided for a magnetical and meteorological observatory in that colony, on the model of the Kew Observatory; and that Professor Meldrum, who has been appointed its superintendent, may be expected immediately at Kew to receive the instruments which have been prepared by Mr. Balfour Stewart, and to make himself acquainted with the details both of instruments and methods in use at that observatory. We have also reason to hope that the example thus set at Mauritius will shortly be followed at Melbourne and at Bombay.

A summary of the results arrived at in discussing the Solar Autographs taken at the Kew Observatory with the Photoheliograph belonging to the Royal Society has appeared in the 'Proceedings;' and the Fellows have thus been made acquainted, in a general way, with the conclusions which have been based on the observations so obtained. The state of the atmosphere permitting, pictures of the sun are taken daily by Miss Beckley, daughter of the resident mechanical assistant; and these are as regularly measured and discussed by Dr. Loewy. In this way has been accumulated a vast mass of materials on which to found conjectures as to the nature of the physical forces operating at the surface of the sun; and, taking these materials as a basis, Messrs. De la Rue, Stewart, and Loewy have drawn the conclusions enunciated in their several papers on solar physics. It is, however, by no means improbable that other investigators, could they obtain access to the same full and complete details of the observations and measurements, would succeed in evolving other and most important theories of solar activity, and thus that our knowledge of the subject might be greatly advanced. It is moreover evident that in a method of observation so new, and in a subject so intricate, the minutest fact can hardly be dismissed as insignificant, seeing that, whatever its present apparent isolation, it may hereafter be shown to stand connected with an important series of facts, towards a right theory of which it may indeed furnish important aid. It has therefore to be considered in what way the publication of these voluminous details can be best effected. Pending this, however, I am glad to state that the authors above-named have themselves determined to print in detail their first paper, and that a sufficient number of copies will be placed at the disposal of the Society for distribution among the Fellows.

The amount of spotted area is being measured; and the elements of the sun's rotation will be calculated from the spots.

Those of the Fellows who are interested in the trial of gun-cotton as a propellant, will be glad to learn that its employment as a charge for the Whitworth and Enfield Rifles is progressing favourably. By a mode of construction of the cartridge ingeniously devised to control the too great rapidity of combustion, the cotton is found to command, without injury to

the rifle, a range fully equal to that of powder, and, in experiments at the School of Musketry at Hythe, under the superintendence of Major-General Hay, has made excellent shooting, producing diagrams at 1000 yards, hardly, if at all, inferior to those obtained from the best small-bore rifles of the day. These diagrams were obtained with a Whitworth Rifle: in the first, 10 consecutive shots were fired at 1000 yards, with a mean radial deviation of 1·65 foot; in the second, 9 consecutive shots at 1000 yards, giving a mean radial deviation of 2·02 feet. And in the third, 20 consecutive shots were fired at 1000 yards, giving a mean radial deviation of 2·43 feet. The charge in all cases was 25 grains of gun-cotton, the angle varying from 3° to 3° 3′.

The cartridges with which these shots were fired were made *by hand*; the defect of cartridges so made is obvious, viz., that they may not be strictly uniform. But this is an inconvenience remediable by the employment of very simple machinery.

In preliminary trials above 2000 rounds have been fired out of one and the same rifle, without occasioning the slightest injury to the piece.

The advantages of the cotton charges were manifest in the diminution of recoil and smoke, and in the entire absence of fouling.

The demand for cotton charges for sporting-purposes has become very considerable since the shooting-season commenced, and they are understood to have given very general satisfaction.

It is not unreasonable to anticipate that the principles of construction of the cartridges which have proved so successful in the adaptation to small arms, may eventually, with suitable modifications, make cotton available for iron ordnance, as a substitute, in a greater or less degree, for powder, which is far more dangerous in manufacture and storage. As far as has been yet tried, the cotton is found to keep perfectly well for any length of time submerged in distilled water.

I proceed to the award of the Medals:—

The Council has awarded the Copley Medal to M. Michel Chasles, For. Mem. R.S., for his Historical and Original Researches in Pure Geometry.

The historical and original researches of Chasles extend over a period of about forty years. Throughout this time he has devoted his energies, with a constancy of purpose rarely equalled, to the restoration and extension of those purely geometrical methods which, bequeathed to us from antiquity, had their growth arrested during the middle ages, and their utility temporarily eclipsed by the brilliant discovery of coordinate geometry by Descartes. In his well-known 'History of the Origin and Development of Geometrical Methods,' published in 1837 and crowned by the Academy of Brussels, Chasles thus expresses what has proved to be the leading object of his life's labours:—

"I propose to show, so far as my feeble means will permit, that in a multitude of questions the doctrines of pure geometry most frequently present to us that easy and natural path which, penetrating to the very

origin of truths, brings us into actual contact with each individual truth, and at the same time reveals to us the mysterious chain by which all are connected."

The elaborate work here quoted* is unique of its kind; it is our highest authority on all matters connected with the history of geometry, of which science it carefully traces the development from the time of Thales and Pythagoras, down to the earlier part of the present century. Although professing to be an *aperçu* merely, it nevertheless represents a vast amount of historical research, and is moreover enriched by copious notes containing the results of important original investigations.

In the year 1846 the foundation of a chair of modern geometry was decided upon by the Faculty of Sciences at Paris, and Chasles was at once chosen to supply a demand which his own researches had in a great measure created. Thus commenced that personal influence on the younger geometers of his country which still continues, and is traceable in all their productions. Another result of this appointment, by which geometers of all nations have greatly profited, was the publication, in 1852, of his 'Treatise on the Higher Geometry'†,—a work in which the three fundamental principles of pure geometry are, for the first time, fully and systematically expounded. These principles embrace the modern theories of anharmonic ratios, of homographic divisions and pencils, and of geometric involution. An anharmonic ratio is in reality a ratio of two ratios, the latter having reference to two pairs of segments determined by any four points of a line. On one peculiar property of this ratio—that of its remaining unaltered by projection—all modern geometry may be said to be founded. Homographic divisions consist of two rows of points, in the same straight line or in different ones, which so correspond that the anharmonic ratio of any four points of one row is equal to that of the corresponding points of the other row. Finally, two homographic rows, in the same straight line, are said to form an involution when to any point whatever of that line one and the same point corresponds, no matter to which of the two rows the first point may be conceived to belong. Usually there are two points in such an involution, each of which coincides with its own corresponding point; by a mere accident of position, however, the actual existence of these *double points* may be destroyed, whilst all other properties of the involution remain intact. In this contingency originated a mode of speech of the greatest utility in geometry. The double points are said to be *real* in the one case, and *imaginary* in the other. For the undisguised and philosophic introduction of imaginary points and lines into pure geometry we are chiefly indebted to Chasles.

* Aperçu historique sur l'origine et le développement des méthodes en Géométrie, particulièrement de celles qui se rapportent à la Géométrie Moderne; suivi d'un Mémoire de Géométrie sur deux principes généraux de la science, la dualité et l'homographie. Bruxelles, 1837. German translation by Dr. Söhncke; Halle, 1837.

† Traité de Géométrie Supérieure. Paris, 1852.

The term anharmonic ratio, now universally employed, is due to Chasles; the ratio itself, however, appears to have been known to Pappus, the eminent Alexandrian geometer of the fourth century. Chasles, indeed, has shown that this ratio probably constituted an essential feature of those three famous books on Porisms, which Euclid is known to have written, but of whose nature vague indications merely have been transmitted to us in the mathematical collections of Pappus. Robert Simson of Glasgow, the well-known translator of Euclid's 'Elements,' was the first who satisfactorily solved the enigma concerning the real nature of Porisms, and he also succeeded in partially restoring the three lost books. Chasles, however, was the first to restore them completely; and this he has done in a work* which is admitted to be a valuable addition to the history of geometrical science, as well as a model of ingenious and philosophical divination.

Chasles has contributed to the advancement of pure geometry, not only by means of the three complete works already alluded to, but also through the publication of numerous smaller memoirs. Of these the following, by no means the only important ones, demand a passing reference.

The papers on "Stereographic Projections" converted a method originally devised for the construction of maps into a powerful instrument of geometrical transformation. Two able memoirs on "Cones of the Second Order" and on "Spherical Conics," thanks to the translation, published in 1841, by Dr. Graves of Trinity College, Dublin, had a direct influence on pure geometry in our own country. A paper "On the Correspondence between Variable Objects" furnished us with a principle of the greatest utility in all higher geometrical investigations. In several other memoirs the method of generating curves of higher orders by means of homographic pencils of curves of inferior orders is perfected, and new properties are thereby deduced of plane curves of the third and fourth orders. The theory of non-plane curves, especially those of the third and fourth orders, had its origin, for the most part, in Chasles's memoirs; and the modern science of kinematics is indebted to him for two valuable papers on the finite and infinitesimal displacements of a Solid Body. The problem of the attraction of Ellipsoids, rendered celebrated by the investigations of Newton, Maclaurin, Ivory, Legendre, Lagrange, and Laplace, received from Chasles its first complete *synthetical* solution. In this problem, too, originated the conception of confocal surfaces of the second order, the theory of which he has since greatly perfected.

The first volume of Chasles's fourth work (a Treatise on Conic Sections†) appeared during the present year: it is a sequel to his 'Higher Geometry;'

* Les trois livres de Porismes d'Euclide, rétablie pour la première fois, d'après la notice et les lemmes de Pappus, et conformément au sentiment de R. Simson sur la forme des énoncés de ces propositions. Paris, 1860.

† Traité des Sections Coniques, faisant suite au traité de Géométrie Supérieure. Paris, 1865.

and in it the three principles already alluded to find their most appropriate field of application. The second volume of this treatise is looked forward to with interest, as it will contain a full exposition of the admirable researches on conic sections wherewith Chasles has just crowned his labours. These researches, a brief account of which appeared during the past year in the pages of the 'Comptes Rendus,' have put us in possession of an entirely new method, the nature and utility of which may be rendered intelligible even to those who have not made modern geometry a subject of special study.

For the determination or construction of the curves usually called conics, and of which the hyperbola, parabola, and ellipse are species, five conditions are requisite and, in general, sufficient. The nature of these five conditions may be such, however, as to admit of their being satisfied by more than one conic. For instance, although one conic only can be described through five given points, there exist two distinct conics, each of which passes through four given points, and touches a given line. Hence arises the important general question, *How many conics are there capable of satisfying any five conditions whatever?* By the new method of Chasles we are enabled to answer this question, hitherto a difficult one, with great facility. Starting from the elementary cases where the five conditions are of the simplest possible kind, consisting solely of passages through points and contact with lines, he gradually replaces those conditions by more complex ones, and finally arrives at a simple symmetrical formula which fully answers the above question. Seeing how numerous are the questions in conics which may be ultimately reduced to the one here solved, we may, without exaggeration, assert that in this single formula a great part of the entire theory of conics is virtually condensed.

The method has been aptly termed by its eminent discoverer a method of *geometrical substitution.* It involves the consideration of the properties of a system of conics (infinite in number) satisfying *four* common conditions. Such a system is for the first time defined in a manner closely analogous to that in which curves are distinguished into orders and classes. We merely require to know, *first,* how many conics of the system pass through an arbitrarily assumed point, and, *secondly,* how many of them touch any assumed line. These two numbers or *characteristics,* as they are termed, being once found, all the properties of the system of conics are thereby expressible. For instance, the sum of twice the first characteristic and three times the second gives us the order of the curve upon which the vertices of every conic of the system are situated.

This new method of characteristics has been already applied to curves of higher orders, as well as to surfaces; and, considering the magnitude of the new fields of investigation thus opened out, it is probable that, as an instrument of purely geometrical research, the method of Chasles will bear comparison with any other discovery of the century.

Professor Miller,

M. Chasles being prevented from being present in person to receive the Medal which has been awarded to him, I have to request you as our Foreign Secretary to receive it for him, and to transmit it into his hands. It will assure him of the very high estimation in which his labours, in a branch of mathematical research which for more than a century has been little followed and little encouraged, are held in this country.

The Council has awarded a Royal Medal to Joseph Prestwich, Esq., F.R.S., for his numerous and valuable Contributions to Geological Science, and more especially for his papers published in the Philosophical Transactions, on the general question of the Excavation of River Valleys; and on the Superficial Deposits in France and England, in which the Works of Man are associated with the Remains of Extinct Animals.

It is now not less than sixteen years since the Geological Society awarded to Mr. Prestwich the Wollaston Medal, the highest honour in their gift, for the researches and discoveries he had then made; and it may be said without disparagement to the services he had then rendered to geology, that the works he has since completed and published greatly outweigh in amount and value what he had achieved in 1849.

Before that time his writings comprised memoirs both on the palæozoic and tertiary strata:—one on the Old Red Sandstone strata containing ichthyolites, and on some beds of the glacial period at Gamrie; and another, a very elaborate one, on the coal strata of Coalbrook Dale, in which he explained in detail the structure of that coal-field, and the arrangement and distribution of the fossils throughout a long succession of the carboniferous strata. In the tertiary formations he introduced a considerable reform in the classification of the English series by proving, amongst other points, that the central division of the Bagshot Sands coincided in date with the "calcaire grossier" of the Paris Basin, instead of occupying, as was before supposed, a much higher place in the series.

After 1849, continuing his researches on the English tertiary formations, he made two other important steps in the advance of our knowledge, viz., 1st, by showing that the clays of the Island of Sheppey, those of Barton, and those of Bracklesham, in Hampshire, instead of being all three contemporaneous, according to the then received opinions, were each due to a separate period,—an important rectification of the chronological order of the British tertiary formations; and, 2nd, by pointing out that beneath the fluviatile beds of Woolwich, or that series commonly called the plastic clay and sands, there existed an older marine formation, for which he proposed the name of the Thanet Sands—a subdivision now generally recognized and adopted. By establishing the true position of this subdivision, a decided step was made towards filling up the wide gap which still divides the lowest of our Eocene strata from the Maestricht beds or upper part of the chalk.

After completing these and other papers, too many to enumerate here, Mr. Prestwich undertook the more difficult and complicated task of correlating the successive tertiary formations of England, France, and Belgium; and communicated the results in Memoirs published in the Geological Society's 'Journal,' embodying the fruit of many years of travelling and much thought and study.

In 1851 Mr. Prestwich published a separate work on the water-bearing strata around London, facilitating the subterranean search for water by giving actual measurements and probable estimates of the thickness of the chalk and other beds immediately above and below the chalk, and suggesting means of obtaining an additional supply of water for the metropolis.

In 1859 Mr. Prestwich presented to the Royal Society a highly important memoir on the occurrence of flint-implements associated with the remains of animals of extinct species in France and England; and another paper in 1863, on the theoretical questions connected with the same subject.

In these memoirs, as generally throughout all his writings, Mr. Prestwich has exhibited in a very marked degree a combination of unwearied labour and patience in the accumulation of facts, with a remarkable impartiality of judgment in the deduction of their bearing on the existing state of knowledge,—a combination, the value of which cannot be too highly estimated.

Mr. Prestwich,

I present you with this Medal in testimony of the high sense entertained by the Council, and specially by those Members of the Council who are engaged in the same pursuits as yourself, of your laborious researches, and of the spirit in which they have been conducted, in the rectification of many important points in the geology of this and of neighbouring countries, and in tracing out the facts of the occurrence of implements, the work of man's labour, in association with the remains of extinct animals.

The Council has awarded a Royal Medal to Archibald Smith, Esq., F.R.S., for his papers in the Philosophical Transactions and elsewhere on the Magnetism of Ships.

The irregularities to which ships' compasses are liable from the disturbing influence of the iron contained in the ship, originally noticed by the astronomer Wales in the voyages of Captain Cook, and subsequently by Flinders at the commencement of the present century, attained a magnitude in the first of the polar voyages of discovery, viz. that of 1818, which forced on the attention of those who were responsible for the navigation of the vessels the indispensable necessity of meeting and surmounting the difficulties and dangers occasioned thereby. Having been attached to the two first of these expeditions to take charge of all matters of a scientific character, this duty devolved more especially on myself; and before the expedition of 1819 quitted the northern shores of Britain (those of the Shetland Islands), two leading characteristics of modern practice—the

establishment of a standard compass, in a fixed and suitable position, by which compass alone the ship's course should be directed and all bearings should be taken, and the formation of a table of deviations on the several points of the compass by the method now so universally practised of swinging the ship—were adopted in both the 'Isabella' and the 'Alexander.' The systematic character of the deviations, unprecedented in amount, which were experienced by these ships in subsequent parts of their voyage, attracted the attention of an eminent French geometrician, Poisson, who published, in 1824, two papers in the Memoirs of the French Institute, containing a mathematical theory of magnetical induction, with formulæ involving coefficients to be determined by observation, expressing the action of the soft iron of a ship upon her compass—and, in a subsequent memoir, adapted the formulæ to observations made on shipboard sufficient in number to determine the coefficients in the particular case of the soft iron being symmetrically distributed on either side of the principal section of the ship. The application of these formulæ was verified by deviations calculated for different positions in the high northern latitudes, where the absolute values of the magnetic elements, as well as the deviations of the compass on board, had been observed by the polar ships, the observed and calculated deviations showing a remarkable accordance.

About twenty years after the date of the Arctic voyages, the system of compass-correction, which had been so successfully practised in the ships engaged in those voyages, was definitely adopted in the Royal Navy, on the recommendation of a committee appointed by the Admiralty, including among its members two of the officers who had served on these voyages, viz. the late Sir James Clark Ross and myself.

At a somewhat later epoch the Magnetic Survey of the Antarctic regions brought into prominent view the importance and value of Poisson's theory. By far the greater part of the Survey having to be executed by daily observation of the three magnetic elements on shipboard, it became desirable for the deduction of the results, that the fundamental equations of Poisson's theory should receive such a modification as should adapt them to the form in which the data generally present themselves. This was the first great service which Mr. Smith rendered towards the correction of the irregularities occasioned by the magnetism of ships. Himself a mathematician of the first order, and possessing a remarkable facility (which is far from common) of so adapting truths of an abstruse character as to render them available to less highly trained intellects, he derived, at my request, from Poisson's fundamental equations, simple and practical formulæ including the effects both of induced magnetism and of the more persistent magnetism produced in iron which has been hardened by any of the processes through which it has passed. These formulæ supplied the means of a sufficiently exact calculation when the results of the Survey were finally brought together and coordinated. They were subsequently printed in the form of memoranda in the account of the Survey in the

'Phil. Trans.' for 1843, 1844, and 1846. Instances occurred during the Survey, and are recorded in the account, in which (although these were not *iron* ships) the difference of the pointing of the compass on different courses exceeded 90°; the differences almost entirely disappearing when Mr. Smith's formulæ were applied.

The assistance which, from motives of private friendship and scientific interest, Mr. Smith had rendered to myself was, from like motives, continued to the two able officers who have successively occupied the post of Superintendent of the Compass Department of the Navy; and the formulæ for correcting the deviation, which he had furnished to me, reduced to simple tabular forms, were published by the Admiralty in successive editions for the use of the Royal Navy.

As in the course of time the use of steam machinery, the weight of the armament of ships of war, and generally the use of iron in vessels increased more and more, the great and increasing inconveniences arising from compass-irregularities were more and more strongly felt, and pressed themselves on the attention of the Admiralty and of naval officers.

An entire revision of the Admiralty Instructions became necessary. Mr. Smith's assistance was again freely given, and the result was the publication of 'The Admiralty Manual for ascertaining and applying the Deviations of the Compass caused by the Iron in a Ship.'

The mathematical part of this work, which is due to Mr. Smith, seems to exhaust the subject, and to reduce the processes by simple formulæ and tabular and graphic methods to the greatest simplicity of which they are susceptible. Mr. Smith also joined with his fellow-labourer, Capt. Evans, F.R.S., the present Superintendent of the Compass Department of the Navy, in laying before the Society several valuable papers containing the results of the mathematical theory applied to observations made on board the iron-built and armour-plated ships of the Royal Navy.

Owing in great measure to these researches, the system practised in the Navy has been brought to its present advanced state.

The outline of the system may be stated briefly as follows:—

1. As regards the building of ships. It has been ascertained that the amount of disturbance is greatest in iron ships which are built (in British ports) with their heads to the North, and is still further and greatly increased in armour-plated ships when they are plated with their heads in the same direction in which they were built. It is therefore desirable that iron ships should not be built with their heads to the north, and that armour-plated ships should be plated in the reverse position to that in which they were built.

2. In respect to the fitting of ships. It is held to be essential that in every ship a *Standard Compass* should be fixed in a position selected, not for the convenience of the helmsman or of the builder, but for the moderate and uniform amount of the deviation at and around it, and where every facility exists for the examination of errors, by comparison

with the azimuths of celestial objects, or by terrestrial bearings. No iron of any kind should be placed, or should be suffered to remain, within a certain distance of the Standard Compass; in the British naval service this distance is 7 feet: and all vertical iron, such as stanchions, arm-stands, &c., should be at a still greater distance; in the British naval service this distance is 14 feet,—whether on the same deck or immediately below it.

It is not difficult to select a place where the Standard Compass can be most advantageously placed; but it is difficult, and some more stringent measures are required than at present exist, to induce ship-builders to adapt the arrangements of the vessel to the requirements of the compass.

3. In respect to those who have to navigate the ship. Every iron ship should be swung when her cargo is complete, and when she is ready in all respects for sea. Tables of the deviation of the Standard Compass on each course should be made according to the directions now universally adopted in Her Majesty's Navy, the tabular deviations being applied as corrections to the courses steered. The table of deviations to be carefully watched as the ship proceeds on her voyage, by comparison with the azimuths of celestial objects, and reformed as changes in the geographical position of the ship, or in the magnetic condition of her iron, take place, according to rules which have been devised for that purpose, confirmed by experience, and published by authority.

By a strict adherence to the precautions, arrangements, and practices which have been thus briefly sketched, the compass may still, in great measure, retain its place as the invaluable guide to the mariner in iron ships, as it was formerly in wooden ships.

But with the increased employment of iron increased vigilance is required in those on whom the responsibilities devolve. The assiduous labours of several eminent men, and prominently amongst them of Mr. Smith, have placed it in the power of any intelligent seaman to navigate his iron ship with safety; but it cannot be too strongly inculcated, that *no* processes of supposed correction—whether tabular or mechanical—should be allowed to interfere with the habitual and constant practice of examining the Standard Compass, on all occasions when the state of the heavens will permit, by comparisons with celestial objects.

The benefits of Mr. Smith's labours have not been confined to our own Navy. The works to which he has contributed have been translated into the German, Russian, and French languages. The British system has been adopted in Russia, whose vessels have to navigate a sea in which the magnetic dip, and consequently the deviation of the compass, is even greater than in our own seas. A Compass Observatory has been established at Cronstadt to fulfil the same purposes as our Compass Observatory at Woolwich. Amongst our neighbours the French, whose fleets approximate the nearest to our own in the species of defensive armour which is perilous to their navigation, the system adopted in this country to preserve the utility of the compass has been the subject of a special mission appointed

by the Government, and of a Report addressed to the Minister of the Marine by M. Darondeau, entitled "Rapport à son Excellence le Ministre de la Marine sur une Mission accomplie en Angleterre pour étudier les questions rélatives à la régulation des Compas." The principal conclusions of this Report in reference to the compass by which the ship's course is to be directed, may be stated in a few words; and I shall employ for this purpose M. Darondeau's own expressions, as they are a remarkable testimony to the value of the system adopted in the British Navy.

"Établir sur tous les bâtimens un 'Standard Compass,' ou compas de relèvement à poste fixe, *qui ne serait pas corrigé.* Ce compas dévrait être assez élevé pour permettre de prendre les relèvemens par dessus le bastingage; il devrait en outre être placé dans la position la plus favorable pour n'être soumis qu'à la force totale du navire, et non aux forces perturbatrices provenant de pièces de fer isolées. Dans ce but on l'élèverait de manière à le soustraire à ces dernières forces perturbatrices.

"*Ce compas ne serait jamais corrigé.*"

The *italics* are mine; but the repetition of this last injunction is M. Darondeau's own, and is emphasized by him by being made to occupy a line by itself.

M. Darondeau also recommends the employment in the French Marine of compasses similar to the Admiralty compass of the British Navy, having four needles attached to the card in the manner and for the purposes originally suggested by Mr. Smith; and he does not fail to urge on his countrymen the indispensable duty of examining the deviations of the Standard Compass by reference to the heavenly bodies, whenever the state of the weather will permit.

Mr. Smith,

Receive this Medal which the Council has awarded you in testimony of their high sense of the value of your researches on the magnetism of ships.

I trust that you will always regard it with a real pleasure, agreeing well with the yet higher pleasure derived from the consciousness of the essential service your generous labours have rendered to the mariners of this and all other maritime nations.

I will venture on the personal expression of the high gratification which my position in this chair allows me this day to enjoy—in mine being the hand which places this Medal in that of one who from his earliest youth has been the object of my ever-increasing high esteem and warm friendship.

NOTES.

Note A.

The steps which have led to the procurement of a large reflecting telescope for active employment in the southern hemisphere originated in a resolution passed by the General Committee of the British Association assembled at Birmingham in September 1849, during the Presidency of the Rev. Dr. Thomas Romney Robinson. The resolution was as follows:—

"That an application be made to Her Majesty's Government to establish a reflector of not less than 3 feet in diameter at the Cape of Good Hope, and to make such additions to the staff of that observatory as may be necessary for its effectual working; and that the President be requested to communicate with the Earl of Rosse and Sir J. Herschel, the Astronomer Royal, Sir Thomas Brisbane, and Dr. Lloyd on the subject; and to obtain the concurrence in the application of the Royal and Astronomical Societies of London, the Royal Society of Edinburgh, and the Royal Irish Academy."

The communications thus directed having been made, the President and Officers of the British Association received on the 9th of the November following (1849) a reply from the Council of the Royal Astronomical Society, declining to cooperate with the British Association in recommending the establishment of a large reflector at the Cape of Good Hope, on the ground that "a system of observations essentially meridional, as those of the Cape Observatory now are, has very little in common with a system of observations with a large reflector. The Council conceive that the subjects and methods and difficulties of the last-mentioned observations absolutely require the entire energies of a superintendent fitted by his talents and education to be the head of an observatory. They consider therefore that the proposal in question amounts to nothing less than the establishment of a new observatory, a measure which the Council [of the Royal Astronomical Society] are not prepared to recommend."

The reply of the Council of the Royal Society of Edinburgh was dated December 10, 1849, and was as follows:—"The Council [of the Royal Society of Edinburgh] are of opinion that it is not expedient at present to take part in the proposed application to Government relative to the large reflecting telescope, suggested to be sent to the Cape of Good Hope."

No specific reply appears to have been received from the Royal Irish Academy, it having been stated in a letter from Dr. Lloyd to the Rev. Dr. Robinson, that "the Council of the Royal Irish Academy had declined to enter upon the subject, as not being strictly within the province of the Academy."

The reply from the Royal Society of London was dated April 19, 1850, and was as follows :—

"The President and Council of the Royal Society agree entirely with the British Association in their estimate of the importance of the active use of a large reflector in the southern hemisphere, and deem the subject well worthy of a recommendation to Her Majesty's Government, in which they would be ready to concur; but they would deem it advisable that, in such recommendation, the locality to which the telescope should be sent, and the establishment to which its use should be confided, should be left to the choice of Her Majesty's Government."

These replies were submitted to the Council of the British Association on the 20th of May, 1850, when the Council passed the following resolution :—

"That the object which the General Committee had in view in their resolution for a recommendation to establish a large reflector at the Cape of Good Hope, viz. the systematic observation of the nebulæ of the Southern Hemisphere with an instrument of great optical power, would be accomplished by the establishment of such an instrument in any other part of the Southern Hemisphere which should be equally suitable for the observations in question; the Council are therefore of opinion that the President will be carrying out the spirit of the recommendation of the General Committee, by putting the proposition to be made to Her Majesty's Government in the general form suggested by the President and Council of the Royal Society, and by concurring with the President of the Royal Society in submitting the recommendation so modified to the consideration of Her Majesty's Government."

The President (Dr. Robinson) was further requested to draw up a Memorial to accompany the Resolution, and to communicate thereupon with the Earl of Rosse, President of the Royal Society. The Memorial prepared by Dr. Robinson, and concurred in by the Earl of Rosse, was presented, in accompaniment with the Recommendation of the General Committee thus amended, to Earl Russell (then Lord John Russell), the First Lord of the Treasury. The Memorial itself may be referred to in the "Report of the Council to the General Committee of the British Association assembled at Edinburgh in July 1850." The reply from the Treasury was as follows :—

"Treasury Chambers, August 14, 1850.

"Sir,—I am commanded by the Lords Commissioners of Her Majesty's Treasury to acquaint you that your Memorial of the 3rd ultimo, addressed to Lord John Russell, applying, on behalf of the British Association for the Advancement of Science, for the establishment in some fitting part of Her Majesty's dominions of a powerful Reflecting Telescope, and for the appointment of an observer charged with the duty of employing it in a review of the Nebulæ of the Southern Hemisphere, has been referred by His Lordship to this Board; and I am directed to inform you with re-

ference thereto, that while My Lords entertain the same views as those expressed by you as to the interest attaching to such observations, yet it appears to their Lordships that there is so much difficulty attending the arrangements which alone could render any scheme of the kind really beneficial to the purposes of science, that they are not prepared to take any steps without much further consideration.

"I am, Sir, &c. &c.,

"G. Cornewall Lewis."

This reply, though failing to meet the not unreasonable expectations which had been founded on the intrinsic importance of the subject itself and on the earnest recommendation it had received from the two principal scientific institutions of the kingdom, was still so far satisfactory that it conveyed the approval of the Government of the principle of the proposition; it was reasonable to believe therefore that by perseverance and by a judicious selection of times and opportunities the object would be eventually secured. Such was the view taken by its promoters; and in accordance with this view the subject was again brought under the consideration of the British Association at their Meeting at Belfast in September 1852, in the opening address of the President, suggesting that a decision should be taken—whether any, and if any, what official step should be adopted for its immediate furtherance. After the usual discussions in Sections and Committees, the General Committee passed the following Resolution:—

"That it is expedient to proceed without delay in the establishment in the Southern Hemisphere of a Telescope not inferior in power to a 3-feet Reflector; and that the President (Col. Sabine), with the assistance of the following gentlemen, viz. the Earl of Rosse, Dr. Robinson, Lord Wrottesley, Professor Adams, the Astronomer Royal, J. Nasmyth, Esq., Wm. Lassell, Esq., Sir D. Brewster, and E. J. Cooper, Esq., be requested to take such steps as they shall deem most desirable to carry this resolution into effect."

The first step taken by this Committee was to communicate the resolution to the President (The Earl of Rosse) and Council of the Royal Society, who (on the 25th of November, 1852) resolved as follows:—

"That the President and Council agree with the British Association in considering it desirable to proceed without delay in obtaining the establishment of a Telescope of very great optical power for the observation of Nebulæ in a convenient locality in the Southern Hemisphere; and that a Committee be appointed to take such steps as they may deem most desirable to carry out this resolution. The Committee to consist of the President, Officers, and Council of the Royal Society, with the addition of Sir John Herschel, Sir John Lubbock, and the Dean of Ely." It was also agreed that the Committee should act conjointly with the gentlemen named in the Resolution passed by the British Association.

The joint Committee applied themselves in the first instance to a con-

sideration of the most suitable construction and dimensions of a telescope for the desired purpose. This was effected by a correspondence amongst the members of the Committee, passing through the Secretary of the Royal Society, the letters being printed for greater convenience in circulation. The proceedings of this Committee were terminated by a meeting of its members at the apartments of the Royal Society on July 5, 1853, the Earl of Rosse, President, in the Chair; when the following resolutions were passed:—

"1. That the Committee approve the proposition made by Mr. Grubb, and contained in Dr. Robinson's letter of June 30, 1853, for the construction of a *four*-foot Reflector.

"2. That application be made to Her Majesty's Government for the necessary funds.

"3. That the Presidents of the Royal Society and of the British Association, accompanied by Dr. Robinson, who was associated with the Earl of Rosse in the former application, and Mr. Hopkins, the President elect of the British Association, be a deputation to communicate with Government respecting the preceding Resolutions.

"4. That the Earl of Rosse, Dr. Robinson, Mr. Warren De la Rue, and Mr. Lassell be a Subcommittee for the purpose of superintending the progress of Mr. Grubb's undertaking."

No record appears to have been made of the subsequent steps taken by this Committee; but it is understood that the application was made to the Earl of Aberdeen, who had become First Lord of the Treasury, and that the reply received was that "no funds could be then spared as the country was engaged in the Crimean war; but that when the crisis then impending was past the matter should be taken up." Lord Aberdeen's retirement from office, and subsequent death, rendered this promise of no avail.

I must now advert to a circumstance which has exercised a most beneficial influence on the proposition for a southern telescope, and has contributed greatly to bring it to its present advanced stage. Amongst the Members of the Mathematical and Physical Section of the British Association who took part in the discussions relating to the telescope at the Belfast Meeting, there was one, Mr. William Parkinson Wilson, Professor of Mathematics in Queen's College, Belfast, who was remarked for the deep and earnest interest with which he viewed the subject. Appointed shortly afterwards to the Mathematical Chair in the University of Melbourne, Professor Wilson appears to have been impressed by the suitability of Melbourne for such a telescope, both from its latitude and climate, and from the increasing wealth and public spirit of its inhabitants manifested in the liberal support given to many scientific institutions. Melbourne enjoyed also at that time the great advantage of a Governor, Sir Henry Barkly, whose education and acquirements enabled him to appreciate the importance in such a colony of scientific cultivation. Being appointed Hon. Secretary of the Board of Visitors of the Melbourne Observatory,

then in process of organization, and with the sanction of the Governor, who was President of the Board, Professor Wilson submitted to the consideration of the Observatory Committee of the Philosophical Institute of Victoria a scheme for the establishment at Melbourne of a reflecting telescope of 4 feet aperture to carry out the objects which had been proposed by the Royal Society and British Association, as already narrated. In this proposition Professor Wilson was warmly supported by Captain Kay, R.N., F.R.S., one of the Board of Visitors, who had been for several years Superintendent of the Magnetical and Meteorological Observatory in the sister colony of Tasmania. After discussions at several Meetings, a Memorial was adopted and presented to the Chief Secretary of the Government, adverting to the favourable condition of the finances of the colony, and urging the establishment of such a telescope at Melbourne "as suited alike to render an important service to science, and to redound highly to the credit of the colony, both in Australia and in Europe."

The favourable reception of this Memorial by the Government of Victoria, and the proceedings which followed, will be best explained by the following despatch from Sir Henry Barkly to the Duke of Newcastle, then Secretary of State for the Colonies, transmitted to the Royal Society on October 10, 1862, accompanied by the expression of His Grace's assurance that "the Royal Society would do whatever may be in their power for encouraging science in the colony of Victoria."

Governor Sir H. Barkly to the Duke of Newcastle.

(Copy.)

Government Offices, Melbourne,
23rd July, 1862.

My Lord Duke,

The Board of Visitors to the Melbourne Observatory, over which I have the honour to preside, being of opinion that the project long entertained of erecting in the Southern Hemisphere a telescope of much greater optical power than that used by Sir John Herschel at the Cape of Good Hope, would be materially advanced by an expression of interest and sympathy on the part of scientific men in England, has requested me to bring the subject under Your Grace's notice, with a view to its being submitted for the Report of the Royal Society of London and the British Association for the Advancement of Science.

I have great pleasure in forwarding accordingly, with the approval of my advisers, an extract from the Board's Minutes, together with the accompanying letter from its Honorary Secretary, Professor Wilson, in which the reasons for this step are so fully set forth, and the advantages likely to arise from obtaining a powerful instrument for this purpose so clearly explained, as to leave nothing for me to add beyond earnestly soliciting Your Grace's good offices in the matter.

It will be observed that the pecuniary cooperation of the British

Government is not applied for; but I need hardly say that even the smallest donation from that quarter would much facilitate raising the necessary funds.

I avail myself of this opportunity to put Your Grace in possession of the Second Annual Report of the Board of Visitors, from which it will be found that a commencement has been made in the erection of the new Observatory, advocated in the Report previously transmitted; and I am glad to be able further to state that a sum of £4500 has since been voted by the Legislature for the completion of the requisite buildings.

Should it be possible, therefore, to add an equatorially mounted telescope, the Astronomical Branch of the Observatory will be rendered complete, and no greater expense than at present will be incurred for the Staff attached to it. I have, &c.,

(Signed) HENRY BARKLY.

His Grace the Duke of Newcastle, K.G.,
&c. &c. &c.

Professor Wilson to Sir H. Barkly.

(Copy.)

The University, Melbourne,
16th July, 1862.

SIR,

I have the honour, by direction of the Board of Visitors to the Observatories, to forward to Your Excellency the accompanying extract from the Minutes of a Meeting held yesterday, and to express a hope that you will comply with the request contained in it.

Though entertaining no doubt of the importance of the results to be obtained by such a telescope as is recommended, or of the conspicuous and creditable position which Melbourne would consequently occupy in the eyes of all persons in Europe who take an interest in Science, the Board is desirous of obtaining an expression of opinion from scientific men in England, because it is due to those who may be asked to contribute towards its accomplishment that the importance of the object should be attested by higher scientific authority than the Board can lay claim to; because also it considers that every means should be used to obtain, so far as funds will permit, the best instrument which modern skill and recent inventions render possible; because, finally, the Board feel that, whether the cost of the instrument be defrayed wholly or partially by private contributions or a grant from the Legislature, public sympathy will be much more strongly enlisted in its favour by a statement of the interest taken in the matter in Europe, and by the approval of the Imperial Government, than by any representation which the Board can make.

I have, &c.,

(Signed) W. P. WILSON,
Secretary to the Board of Visitors.

His Excellency the Governor.

Extract from the Minutes of a Meeting of the Board of Visitors to the Observatories, held 15 *July* 1862.

"The attention of the Board having been drawn to the following circumstances—

"I. That, as long since as 1849 the facts brought to light by Lord Rosse's Telescope were judged by the Royal Society of London and the British Association for the Advancement of Science to be so important as to justify them in making an urgent appeal to the British Government for the erection, at some suitable place in south latitude, of a telescope for the examination of the multiple stars and the nebulæ of the Southern Hemisphere, having greater optical power than that used by Sir John Herschel at the Cape of Good Hope; which appeal there is little doubt would have been successful but for the Russian war and the consequent expenditure;

"II. That, since that time, Lord Rosse reports that he has discovered systematic changes in some of the most important northern nebulæ;

"III. That the interest and scientific importance of the solution of the problem of their physical structure, as well as the probability of its accomplishment, are thus greatly increased;

"IV. That some of the most important nebulæ, and those presenting the greatest variety of physical features in close proximity, can be observed only in places having a considerable southern latitude;

"V. That the geographical position and clear atmosphere of Melbourne render it peculiarly suitable for this work, and that the arrangements already made for the establishment of an Astronomical Observatory on a permanent footing offer great facilities for carrying it on;

"VI. That, independently of the especial object to which such telescope would be applied, an Astronomical Observatory cannot be considered complete without an equatorially mounted telescope of large optical power:

"It was Resolved,—

"1st. That, in the opinion of the Board, the establishment of such a telescope in Melbourne would materially promote the advancement of science.

"2nd. That, before applying to the Colonial Government for any pecuniary grant in aid of this object, His Excellency the Governor be requested to obtain, through the Secretary of State for the Colonies, an expression of opinion from scientific men in England as to the importance of the results to be expected from it, the most suitable construction of telescope for the purpose, both as to the optical part and the mounting, its probable cost, and the time requisite for its completion."

On the receipt of this communication from the Colonial Office, a correspondence ensued, passing through myself as President of the Royal Society, consisting of twenty-three letters, the writers being Mr. Lassell, Sir John Herschel, the Earl of Rosse, Dr. Robinson, Mr. Grubb, and Mr. De la Rue, which was printed for private circulation amongst the Fellows of the Royal Society. The correspondence led to and terminated in the following Report

from the President and Council addressed to the Duke of Newcastle, in reply to His Grace's communication of October 10, 1862:—

"*Report of the President and Council of the Royal Society respecting the proposal of erecting in Melbourne a Telescope of greater optical power than any previously used in the Southern Hemisphere.*

"1. The President and Council learn with pleasure that the Board of Visitors at the Melbourne Observatory have proposed resolutions, indicating their sense of the importance of erecting at Melbourne an equatorially mounted Telescope of great optical power, and that the proposal is favourably regarded by Sir Henry Barkly, Governor of Victoria, and by His Grace the Secretary for the Colonies. In respect to the importance which the President and Council attach to such an undertaking, they need do no more than refer to the fact that in the year 1850 the Royal Society and the British Association for the Advancement of Science presented a joint Memorial to Her Majesty's Government, in which they urged the establishment of such a telescope at some suitable place in the Southern Hemisphere. The scientific objects to be attained thereby are so clearly stated in that Memorial, of which a copy is enclosed, and in the Resolutions of the Board of Visitors of the Melbourne Observatory, in July 1862, that the President and Council feel it unnecessary to do more than refer to these documents.

"2. Since the presentation of the Memorial of 1850, an equatorially mounted telescope of greater optical power than that then recommended has actually been constructed by Mr. Lassell, at his own expense, in England, and erected in Malta, where he is now occupied in making observations with it: we have now, therefore, in addition to our previous knowledge, the benefit of his experience. In referring to Mr. Lassell's Telescope, the President and Council wish it, however, to be understood that they do not conceive that it should necessarily be copied in all respects, and that for the present they think it best to leave the details of construction in many respects open to further consideration.

"3. When the subject was previously under consideration, letters were written to some of the most eminent practical astronomers of Great Britain and Ireland, requesting them to state their opinions as to the best mode of construction; and a correspondence ensued, of which a printed copy is sent herewith. After receiving the communication from the Colonial Office of the 10th of last October, the President wrote to the four gentlemen who were appointed as a Committee on the former occasion to superintend the construction of the instrument (in case the Government should accede to the request), and also to Sir John Herschel, enclosing a copy of the former correspondence, and asking whether their views had in any way changed in the interval. The answers received from each have been circulated among the others, as was done on the former occasion, and have in most cases elicited additional remarks.

"4. Availing themselves of the information thus so kindly afforded

them, the President and Council have to recommend as follows regarding the construction of the instrument contemplated.

"(*a*) That the telescope be a reflector, with an aperture of not less than four feet. This is essential, as no refractor would have the power required.

"(*b*) That the large mirror be of speculum-metal. Such mirrors can be constructed with certainty of success, and at a cost which can be foretold; whereas the recently introduced plan of glass silvered by a chemical process has not yet been sufficiently tried on so large a scale as that contemplated.

"(*c*) That the tube be constructed of open work, and of metal. Lord Rosse has recently changed the tube of his three-foot altazimuth from a close to an open or skeleton one, and it is understood that he intends doing the same with his great telescope. Mr. Lassell's tube is also an open one, which his experience leads him decidedly to prefer.

"(*d*) The telescope should be furnished with a clock-movement in right ascension.

"(*e*) Apparatus for repolishing the speculum should be provided.

"(*f*) With respect to the form of reflector to be adopted, some difference of opinion exists, as the Newtonian and Cassegrainian have each some advantages not possessed by the other. On this point further correspondence appears desirable; but as the main features of the scheme are the same in both cases, there does not appear to be any occasion to wait till this point shall have been finally decided.

"5. With respect to the cost, something must depend on the solidity of the construction and the perfection of the workmanship; but if it be assumed that the workmanship shall be of the best description, and the instrument furnished, as seems desirable, with polishing apparatus, and a second speculum for using while the other is being polished, it is probable that the cost will not fall much short of £5000.

"6. It is estimated that the construction of the instrument will occupy about eighteen months.

"7. It seems highly desirable that the future Observer should come to England during a part at least of the time occupied in the construction of the instrument, in order that he may become thoroughly acquainted with all its details, and especially with the mode of repolishing; and also that he may personally acquaint himself with the working arrangements followed at the Observatories of the Earl of Rosse and Mr. Lassell, who have expressed their willingness to afford him every facility."

This Report, accompanied by several copies of the Correspondence adverted to, was transmitted in due course to Melbourne.

In 1863 Mr. Lassell made the most liberal offer of freely presenting for the observations at Melbourne his own 4-foot reflector, with which he had been carrying on a series of observations at Malta, as soon as that series should be completed, or in the course of a year or two. The construction of this telescope had been largely considered and discussed in the correspondence already adverted to. On Mr. Lassell's munificent offer being

transmitted to Melbourne, the authorities there were at first disposed to embrace it; but subsequently, on further consideration and correspondence, they determined to revert to the original plan, of a telescope to be constructed by Mr. Grubb expressly to meet in the most perfect attainable manner all the special requirements of the case. This plan is described in a letter addressed to Dr. Robinson on the 3rd of December, 1862, being the thirteenth letter in the printed Correspondence referred to. It seems scarcely possible to doubt the wisdom, in every point of view, of the decision thus arrived at. The alterations which would have been required in Mr. Lassell's telescope would have demanded a large proportion of the time and the expense needed for the construction of the new one; and the result would have been that Europe would have lost all the services which Mr. Lassell's telescope may still perform—while Australia would have had a much less perfect instrument, for the especial purposes in view, than it will now possess.

In April 1864 a proposition for a grant of £5000, to cover the expense of constructing a telescope, was submitted to the Colonial Legislature by one of its members, Mr. Alexander John Smith, also a Member of the Board of Visitors of the Observatory, who, previously to his residence in Victoria, had been one of that band of highly-trained naval observers who, under the command of Sir J. C. Ross, had accomplished, between the years 1839 and 1843, the Magnetic Survey of the Antarctic regions, and had subsequently become one of the officers employed with Capt. Kay in the Magnetical and Meteorological Observatory at Hobarton. This proposition was successful; and the notification received from Professor Wilson is printed in the text of this Address, p. 483.

Note B.

The number of hourly tabulations from the photographic traces of the bifilar magnetometer at Kew, between January 1, 1858, and December 31, 1864, is 60,491: of these, the number in which the amount of disturbance from the normal of the same year, month, and hour equalled or exceeded 0·150 division of the scale, or ·0015 of the total horizontal force at Kew, was 5932, being about one in ten of the whole number of tabulated hourly values. The aggregate value of the 5932 disturbed observations in parts of the bifilar scale, of which 1 inch equals ·01 of the whole horizontal force, was as follows:—

Year ending December 31,	1858	267·893 inches.
„ „	1859	369·286 „
„ „	1860	270·349 „
„ „	1861	206·748 „
„ „	1862	183·645 „
„ „	1863	114·642 „
„ „	1864	114·725 „

The *mean* annual value in the seven years is 218·184 inches; and the ratios of disturbance, in each of the seven years, to the mean annual value are as follows:—

Year ending December 31,	1858	1·23
„ „	1859	1·69
„ „	1860	1·24
„ „	1861	0·95
„ „	1862	0·84
„ „	1863	0·53
„ „	1864	0·53

Note C.

Mean Annual Values of the Magnetic Inclination at Toronto deduced from the Monthly Determinations; reprinted from Table LIII. (p. 93) of the 'Abstracts of Observations made at the Magnetic Observatory at Toronto,' published by its Director, G. T. Kingston, Esq. The years 1863 and 1864 are added from the Numbers of the 'Canadian Journal of Science.'

"The monthly determinations were commonly made on three consecutive days, as nearly as possible about the middle of the month. One determination was usually made each day between noon and I P.M. The monthly and annual means were derived directly from the observations."

Years.	1853.	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.	1863.	1864.
Yearly Means. 75°+	22′·17	22′·96	23′·54	24′·06	24′·32	24′·44	24′·98	24′·55	23′·75	23′·19	21′·47	20′·93

On the motion of Mr. Warren De la Rue, seconded by Colonel Yorke, it was resolved,—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The Statutes relating to the election of Council and Officers having been read, and Mr. De la Rue and Mr. Merrifield having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.—{ William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L. }

Foreign Secretary.—Professor William Hallows Miller, M.A.

Other Members of the Council.—John Frederic Bateman, Esq.; Lionel Smith Beale, Esq., M.B.; William Bowman, Esq.; Commander F. J. Owen Evans, R.N.; Edward Frankland, Esq., Ph.D.; Francis Galton, Esq; John Peter Gassiot, Esq.; John Edward Gray, Esq., Ph.D.; Thomas Archer Hirst, Esq., Ph.D.; Sir Henry Holland, Bart., M.D., D.C.L.; William Odling, Esq., M.B.; Sir John Rennie, Knt.; Prof. Warington W. Smyth; William Spottiswoode, Esq., M.A.; Paul E. Count de Strzlecki, C.B., D.C.L.; Vice-Chancellor Sir W. P. Wood, D.C.L.

The thanks of the Society were voted to the Scrutators.

Receipts and Payments of the Royal Society between December 1, 1864, and November 30, 1865.

Receipts	£	s.	d.
Balance at Bank, and on hand	683	14	1
Annual Subscriptions and Compositions	1771	16	0
Rents	249	8	5
Dividends on Stock	963	10	11
Ditto, Ditto, Trust Funds	281	17	6
Ditto, Ditto, Stevenson Bequest	512	11	10
Sale of Transactions, Proceedings, &c.	308	16	9
Chemical Society, Proceedings, 1863–64	50	0	0
Tea Expenses and Gas, repaid	43	6	0
Prof. Cayley, repaid to Donation Fund	15	0	0
Parcel Charges recovered	1	18	7
	£4882	0	1
Balance due to Bankers	135	7	10
	£5017	7	11

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £126 0s. 0d. per annum.

Estate at Acton, Middlesex (34 A. 3 R. 11 P.), £110 0s. 0d. per annum.

Fee farm rent in Sussex, £19 4s. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per Cent. Annuities.

£28,969 15s. 7d. Consolidated Bank Annuities.

£513 9s. 8d. New $2\frac{1}{2}$ per Cent. Stock.

Payments	£	s.	d.	£	s.	d.
Salaries, Wages, and Pension				1023	17	0
£1000 Consolidated Bank Annuities, bought at $90\frac{3}{8}$				905	0	0
The Scientific Catalogue				235	9	10
Books for the Library and Binding				362	7	10
Printing Transactions and Proceedings, Paper, Binding, Engraving, and Lithography				1711	10	9
New Bookcases, Painting, and Repairs				127	3	1
Coal and Lighting				109	1	2
Tea Expenses				46	15	2
Fire Insurance				42	1	6
Shipping Expenses				4	13	11
Taxes				15	1	3
Law Expenses				47	7	10
Stationery				10	6	6
Miscellaneous Expenses				48	0	3
Postage, Parcels, and Petty Charges				33	12	2
Advertising				14	10	6
Subscription:—Mablethorpe Schools				2	2	0
Rumford Fund	138	5	10			
Donation Fund	75	0	0			
Wintringham Fund	35	0	6	259	19	9
Copley Medal Fund	4	15	0			
Prof. Roscoe, Bakerian Lecture	4	0	0			
Dr. Beale, Fairchild Lecture	2	18	5			
Rev. T. S. Evans, Croonian Lecture				2	18	5
				5001	18	11
Balance of Catalogue Account				11	17	3
,, Petty Cash Account				3	11	9
				£5017	7	11

WILLIAM ALLEN MILLER,

Treasurer.

Scientific Relief Fund.

Investments up to July 1865, New 3 per Cent. Annuities £6052 17 8

£6052 17 8

Dr.

	£	s.	d.
To Balance, Subscriptions and Dividends	1129	0	8
	£1129	0	8

Cr.

	£	s.	d.
By Grants	275	0	0
Purchase of Stock	672	10	0
Balance	181	10	8
	£1129	0	8

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1865.

		£	s.	d.
Annual Subscriptions		1131	16	0
Admission Fees		180	0	0
Compositions		460	0	0
Rents		249	8	5
Dividends on Stock (exclusive of Trust Funds)		963	10	11
,, on Stevenson Bequest		512	11	10
Sale of Transactions, Proceedings, &c.		308	16	9
Chemical Society, for Proceedings, 1863–64		50	0	0
Chemical Society, Tea Expenses	£15 11 8	31	3	4
Linnean Society, Tea Expenses	15 11 8			
Geographical Society, Gas at Evening Meetings	8 4 8	12	2	8
Cambridge Local Examination Committee, Gas	3 18 0			
Parcel Charges recovered		1	18	7
Income available for the Year ending Nov. 30, 1865		3901	8	6
Expenditure in the Year ending Nov. 30, 1865		3834	0	9
Excess of Income over Expenditure in the Year ending Nov. 30, 1865		£67	7	9

		£	s.	d.
Salaries, Wages, and Pension		1023	17	0
The Scientific Catalogue		235	9	10
Books for the Library		283	7	10
Binding ditto		79	0	0
Printing Transactions, Part III. 1864, and Part I. 1865	497 6 5	1711	10	9
Ditto Proceedings, Nos. 68–77	305 4 1			
Ditto Miscellaneous	65 14 6			
Paper for Transactions and Proceedings	347 16 6			
Binding and Stitching ditto	105 9 5			
Engraving and Lithography	389 19 10			
New Bookcases, Painting, and Repairs		127	3	1
Miscellaneous Expenses		48	0	3
Coal and Lighting		109	1	2
Tea Expenses		46	15	2
Fire Insurance		42	1	6
Subscription:—Mablethorpe Schools		2	2	0
Shipping Expenses		4	13	11
Taxes		15	1	3
Law Expenses		47	7	10
Stationery		10	6	6
Advertising		14	10	6
Postage, Parcels, and Petty Charges		33	12	2
		£3834	0	9

WILLIAM ALLEN MILLER,

Treasurer.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Having compounded.	Paying £2 12*s.* annually.	Paying £4 annually.	Total.
November 30, 1864.	6	49	320	3	276	654
Since elected	+1	......	+9	......	+10	+20
Since deceased	−1	−2	−20	......	−10	−33
Since withdrawn ..	......	......	......	......	−1	−1
Since defaulter	......	......	......	......	−1	−1
November 30, 1865.	6	47	309	3	274	639

Further Correspondence between the Board of Trade and the Royal Society, in reference to the Magnetism of Ships, and the Meteorological Department *.

Mr. Farrer to General Sabine.

"Board of Trade, Whitehall, 25th July, 1865.

"Sir,—I am directed by the Lords of the Committee of Privy Council for Trade to acknowledge the receipt of your letter of the 25th May, and its inclosed Memorandum, calling attention to the subject of the adjustment of compasses in iron vessels.

"The Memorandum states that the subject of the deviation of compasses is one which has hitherto been regarded as too intricate and obscure to be made the subject of practical rules for seafaring men, but that recent experience has placed the science on a sound basis, and has made it possible to frame rules which there will be no practical difficulty in applying.

"The Memorandum further intimates what those rules should be with respect to the placing and adjustment of compasses, and suggests that measures should be taken by the Board of Trade to enforce their observance. It also suggests that steps should be taken to compel Merchant Officers to become acquainted with them; and finally recommends that for the accomplishment of these purposes an Officer should be appointed, whose duty it should be, in communication with the Compass Department of the Admiralty, to aid the Board of Trade in carrying it into effect.

* Published by order of the Council.

"The Board of Trade desire me in reply to return their thanks to the Royal Society for calling attention to a subject which is of first-rate importance to the Mercantile Marine. They have no doubt that the present practice is far from satisfactory; nor do they think that the steps taken by the Board of Trade under the provisions of existing Acts are such as to remedy the evil. At the same time they can see considerable difficulty in adopting all the suggestions made by the Royal Society.

"The steps which the Board of Trade now take are as follows:—

"The Merchant Shipping Act provides that the compasses of passenger steamers shall be adjusted to the satisfaction of the Board of Trade Surveyors, and according to regulations laid down by the Board of Trade. This duty the Surveyors do as well as the means at their disposal enable them to do, and according to regulations which will be found in paragraphs 83 to 86 of the accompanying 'Instructions to Surveyors.'

"As regards the information of Masters and Mates, the Board of Trade have circulated a pamphlet, prepared by Mr. Towson, of Liverpool, which is, no doubt, known to the Royal Society, and have added a general question on the subject to the Examination-papers.

"Under these circumstances it is to be considered whether the Board of Trade can, and whether, if they can, they ought to do more than they d either as regards the proper supply and adjustment of compasses, or as regards the diffusion of information on the subject.

"As regards the first of these points, viz. the proper supply and adjustment of compasses, the Royal Society will, no doubt, concur with the Board of Trade in thinking that it is very undesirable for the Legislature or the Government, except under very exceptional circumstances, to take upon themselves responsibilities which properly belong to shipowners and insurers, or to dictate to those persons the mode in which they shall carry on their business. The proper supply and adjustment of compasses is a matter so material to the safety and success of their undertakings, that motives of self-interest are likely to effect much greater and much better results than could be hoped for by the compulsory interference of a Government Department. These considerations will have to be very carefully weighed before any attempt is made to obtain from the Legislature further powers for the regulation of compasses in merchant ships. And under the law, as it now stands, the Board of Trade do not see what effectual step they can take in the direction pointed out by the Royal Society.

"In the first place, the powers under which they act only apply to passenger steamers, whilst the want which the Royal Society wish to meet is felt just as much in the case of other iron vessels, which are becoming more numerous every day.

"In the second place, the powers of the Board of Trade only extend to obtaining a Certificate 'that the compasses have been properly adjusted.' They do not enable the Board of Trade or its Officers to see that the compasses are good, or to require—what the Royal Society appears to consider

the most important condition of all—that there should be a Standard Compass (in addition to the Steering Compass) so placed as to be free from local attraction.

"This Board cannot, therefore, do what is wanted under the present Acts.

"There is, however, a body, namely, Lloyd's Register Committee, whose proper business it is to see that ships classed by them are seaworthy, and My Lords will refer this part of the subject to them, stating what they hear upon the subject from the Royal Society. This Board will also gladly communicate to Lloyd's any practical rules which the Royal Society can furnish as to the supply, placing, and adjustment of compasses, and as to the effect upon them of different mòdes of construction of the hull of the ship.

"Secondly. As regards the diffusion of information on the subject of compasses, especially among Merchant Officers, the first desideratum appears to be a clear and intelligible Manual or set of directions upon the subject, containing such practical rules as the present state of Science can furnish, and such a statement of the principles as may be necessary for the comprehension of those rules. My Lords will be glad to be informed by the Royal Society if they can put them in the way of obtaining such a Manual. Any expense connected with its preparation will be readily defrayed by the Board of Trade.

"The next step to be taken would be to introduce the subject into places of nautical education. On this the Board of Trade can do nothing except communicate with the Science and Art Department, which they will gladly do on hearing from the Royal Society that such a Manual as above mentioned is in preparation.

"The third step would be to introduce the subject more effectually into Examinations in Navigation, and to have printed questions prepared for the purpose. On this point also the Board of Trade would be glad to know whether the Royal Society can give them information or assistance. One difficulty which will arise will be the difficulty in finding Examiners who have given sufficient attention to the subject, and the first step must probably be to instruct the Examiners themselves. For this purpose also the suggested Manual will be of great importance.

"The steps suggested above may be taken with the aid of the Royal Society, without any such appointment by the Board of Trade of an additional officer as the Royal Society suggest.

"This disposes of most of the important points referred to. There are two which still require notice. The Royal Society propose that the suggested new Officer of the Board of Trade shall assist at inquiries into wrecks, where questions arise concerning the deviation of the compass. Though the Board of Trade are not prepared to appoint a special officer for this purpose, or to commit the inquiry to such an officer, they think that it would be very useful if, in the cases of future inquiries into

wrecks, where important questions concerning compasses are likely to be raised, a person thoroughly acquainted with the subject could attend and give the Court the benefit of his opinion. On this subject the Board will communicate with the Admiralty.

"Lastly, the Royal Society refer to the possible improvement of the science by means of further observations. As regards this, all the Board of Trade could do would be to obtain observations from Masters of merchant ships, in the manner originally proposed by the Royal Society, when the Meteorological Department of this office was established. The whole subject of that department is now under consideration, and this branch of the subject of the Royal Society's letter will be considered in connexion with the rest of that department.

"I have the honour to be,
"Sir,
"Your obedient Servant,
"T. H. Farrer."

"*Major-General Sabine, &c. &c. &c.,*
President Royal Society."

Mr. Fane to General Sabine.

"Board of Trade, Whitehall,
12th August, 1865.

"Sir,—I am directed by the Lords of the Committee of Privy Council for Trade to forward to you the inclosed copy of a letter received from the Secretary to Lloyd's Register, in answer to a communication from this Board relative to the subject of Compasses in Iron Ships.

"I am, Sir,
"Your obedient Servant,
"W. S. Fane."

"*Major-General Sabine, &c. &c. &c.,*
President Royal Society."

(*Inclosure.*)

"Lloyd's Register of British and Foreign Shipping,
2 White Lion Court, Cornhill, 4th August, 1865.

"Sir,—I am directed to acknowledge the receipt of your letter dated 25th ultimo, with its inclosures, relating to the variation &c. of Compasses in Iron Ships, and to acquaint you that it occupied the attention of the Committee of this Society at their Meeting yesterday.

"It appears that it is a subject encompassed with difficulties, and that but little is known at present as to any method which shall ensure satisfactory action of compasses in iron vessels.

"The Committee apprehend therefore that it will not be in their power to take any active steps in the matter; but they will avail themselves of such means as are at their disposal to obtain information on the important

subject thus brought under their notice, and will apprize the Board of Trade Authorities of the result of their inquiries.

"I am, &c.,
(Signed) "Geo. B. Seyfang,
"*Secretary.*"

"*T. H. Farrer, Esq.,*
Secretary, Board of Trade, London."

General Sabine to Mr. Farrer.

"Llandovery, S. Wales, Aug. 28th, 1865.

"Sir,—I beg to acknowledge the receipt of your letter (3027W) of the 12th inst., enclosing copy of a letter received from the Secretary to Lloyd's Register. They shall be duly laid before the Council of the Royal Society, together with your previous letter, at the first Meeting after the recess.

"From inquiries which I have made I have reason to believe that when the proper time shall come a Manual, such as you have referred to, for the instruction and guidance of the builders, fitters, and navigators of the iron ships employed in conveying passengers and merchandize, might be supplied by persons whose sound and practical knowledge qualify them eminently for rendering such a public service; but a work which should satisfy all the requirements referred to in your letter cannot be prepared until the system to be adopted in the Mercantile Marine shall have been, to some extent at least, determined, and then not without the concurrence of the person or persons who should be charged with bringing the system into practical operation.

"The success which has attended the steps taken by the Board of Admiralty to remedy the evils resulting from the disturbance of the compass in Her Majesty's Ships at a time when the science was in a comparatively rudimentary state, is owing to the combination of a proper code of instructions with arrangements for their enforcement under official and competent superintendence, and may be advantageously referred to as a precedent should the Board be disposed to adopt a similar proceeding.

"I have the honour to be, Sir,
"Your obedient Servant,
"Edward Sabine."

"*T. H. Farrer, Esq.*"

Mr. Farrer to General Sabine.

"Board of Trade, Whitehall, 23rd October, 1865.

"Sir,—I am directed by the Board of Trade to acknowledge the receipt of your letter of the 28th August relative to the preparation of a Manual for the guidance and instruction of persons employed in the construction and navigation of iron ships.

"In reply, I am to thank you for your communication, and to observe that the object of this Board, in proposing a Manual of this kind, was, in

the first and chief place, to place in the hands of those interested in shipping, the means of making themselves acquainted with the results of recent observation, which the Royal Society say can now be made available in practice, and the Board of Trade supposed, and still hope, that this may be done without involving the necessity of Government interference with, and supervision over, the Mercantile Marine.

"I have the honour to be, Sir,
"Your obedient Servant,
"T. H. FARRER."

"*Major-General Sabine, &c. &c. &c.,*
President Royal Society."

Mr. Farrer to General Sabine.

"Office of Committee of Privy Council for Trade,
Whitehall, 24th October, 1865.

"SIR,—I am directed by the Lords of the Committee of Privy Council for Trade, to acknowledge the receipt of your letter of the 15th June last, on the subject of the Meteorological Department of the Board of Trade, and to thank yourself and the Council of the Royal Society for the valuable information, advice, and suggestions which it contains.

"The Council of the Royal Society discuss the system of Weather Telegraphy, and recommend that it shall be continued; they approve of the proposal to hand over to the Hydrographer to the Admiralty such part of the observations collected in the Meteorological Department of the Board of Trade as he can make use of in constructing Charts for the use of seafaring men. And they discuss and recommend the adoption of a new system of making and recording Meteorological Observations on land.

"As regards, however, one branch of the subject, viz. Meteorological Observations made at sea, which formed the original object of the Meteorological Department, and the chief subject of the letter from the Royal Society of the 22nd February, 1855, the Board of Trade are not satisfied that they fully understand the present views of the Royal Society.

"Your letter says in answer to Question 1, contained in my letter of the 26th May last, asking 'Are the objects specified in the Royal Society's letter of the 22nd February, 1855, still as important for the interests of Science and Navigation as they were then considered?' that 'The President and Council are of opinion that the objects specified in the Royal Society's letter of 22nd February, 1855, are as important for the interests of Science as they were then considered.'

"And it further says in answer to Question 2, asking 'To what extent have any of these objects been answered by what has already been done by the Meteorological Department?' that 'Much has without doubt been accomplished in the collection of facts bearing on Marine Meteorology; but as no systematic publication of the results has yet been made, the President and Council are unable to reply more specifically.' It is probably for the reasons contained in this answer, that whilst the other sub-

jects above mentioned are fully discussed in your letter, the subject of these Meteorological Observations at sea is scarcely referred to.

"It is, however, essential that the Board of Trade should be rightly informed upon this point before they can determine what steps should be taken with regard to the Meteorological Department. What is the value of the Observations at sea already collected? what steps should be taken to make them useful? and whether any, and, if any, what further observations of the same kind should be collected, are questions which must be answered before any final arrangement can be made with respect to the other points mentioned in your letter. With the view of clearing up these points, the Board of Trade are disposed to suggest the appointment of a small Committee, consisting, say of three or four persons, to examine the whole of the data already collected by the Meteorological Department; to inquire whether any, and what steps should be taken for digesting and publishing them; and also to report whether it is desirable that observations of a similar kind shall still continue to be collected. Such a Committee would also in all probability be able to make valuable recommendations as to the mode in which the business of the Department (if continued) shall be conducted, and as to the form in which the daily weather reports (by whomsoever they may be made) should be published.

"If the Royal Society concur in this suggestion, the Board of Trade would ask them to appoint, as a member of the Committee, some gentleman whose acquirements would enable him to give valuable advice on the scientific part of the subject, and they would also ask the Admiralty to appoint another member. The Board of Trade will feel much obliged if you will favour them with the opinion of the President and Council on this suggestion.

"With reference to the subject of Meteorological Observations on land, the Board of Trade do not clearly understand whether the Royal Society think that they should be substituted for, or be in addition to the Meteorological Observations at sea, which were originally suggested by the Royal Society. They are disposed to agree with the Royal Society in thinking that any observations of a scientific nature would be better conducted under the authority and supervision of a scientific body such as the Royal Society, or the British Association, than of a Government Department. But they do not see how they could advise the Government to sanction any plan which would involve the establishment of two separate Offices for Meteorological purposes, one under the Board of Trade at Whitehall, and the other at Kew. It seems to them obvious that any assistance to be given by Parliament for Meteorological purposes will be more advantageously employed if concentrated at one place, and in one set of hands, than it can be if distributed among different Establishments.

"I have the honour to be, Sir,

"Your most obedient Servant,

"*The President of the Royal Society.*" "T. H. FARRER."

Staff-Commander Evans, R.N., to General Sabine.

"Hydrographic Office, October 23rd, 1865.

"My dear Sir,—I have forwarded to Burlington House for your acceptance, a copy of my letter of suggestions relative to iron ships and their compasses, drawn up for the Board of Trade.

"I gathered from a recent conversation that you were desirous of having this document, with the possible view of showing it to the Council of the Royal Society. I hope it may be found useful, as supplementary to your and their labours."

"I am, my dear Sir,
"Yours very faithfully,
"*General Sabine, &c. &c. &c.*" "Fred. Jno. Evans."

Copy of Letter, with Appendices, from Staff-Commander Evans, R.N., to the Hydrographer of the Admiralty.

"Admiralty, Hydrographic Department, September 1865.

"Sir,—Having carefully examined the correspondence between the President and Council of the Royal Society and the Board of Trade on the Magnetism of Ships, together with the Memorandum appended to the President's letter of the 18th May, and having also considered the requisitions made by the Board of Trade to the Admiralty, by letter of the 28th July, 1865, to be furnished through the Compass Department with any information or suggestions on the subject, I have to submit the following for your consideration.

"The Memorandum of the Royal Society is so comprehensive in its general views of the subject, that little remains to be added to the arguments and reasons therein advanced; but in those matters of detail which would require attention in the event of action being taken on the recommendations of that body, there are several suggestions which present themselves, and which possibly may be useful to the Board of Trade. To these I address myself.

"To clearly understand the existing difference of administration, in compass-equipment and efficiency, between the Royal and Mercantile Marine, it is necessary to point out the views the Board of Admiralty entertained, and the steps they deemed it necessary to take on the introduction of steam machinery, and of so much iron in the general construction of ships of the Royal Navy.

"Passing over the investigations successively made under their auspices by Flinders in 1814, Barlow in 1821, and Johnson in 1836 the Admiralty in 1837, 'deeming it necessary to apply some remedy to an evil so pregnant with mischief,' referring to the then defective state of the compasses supplied to Her Majesty's ships, 'have determined to have the subject fully investigated by a Committee of Officers conversant with magnetic instruments.' Resulting from the labours of this Committee, which extended

over several years, was not only the improvement of the compass itself, but the establishment of a system of compass-adjustment which has since been uniformly followed in Her Majesty's Navy.

"The principal features of this system are the following:—

"1. The having in each ship a standard compass distinct from the steering-compass, fixed in a position selected, not for the convenience of the helmsman, but for the moderate and uniform amount of the deviation at and around it, by which compass alone the ship is navigated.

"2. The requiring each ship to be swung, and to be navigated by a Table of Errors.

"The Admiralty further at this period (1842), to ensure the proper manufacture and adjustment of the standard compass, especially the selection of its position in the ship, and the general supervision of the 'swinging' of the ships of the Fleet, created a small Compass Department, and erected an Observatory and offices for the general examination of all the compasses supplied to Her Majesty's ships. As a matter of opinion, I may here express my belief that indirectly this latter establishment has tended very much to the improvement of compasses generally.

"The Admiralty at this time also issued a small book of Rules, known as the 'Practical Rules' for ascertaining and applying the deviations of the compass; these Rules have undergone revision and addition from time to time. (The latest edition is appended.)

"General rules were also now laid down for guarding, in the equipment of the ship, against the near proximity of iron to the compass: extracts embracing the leading features of these Rules will be found in Appendix 1.

"In 1862, consequent on the increased use of iron in the construction and armature of ships of war, there was issued for the service of the Fleet, the 'Admiralty Manual of the Deviations of the Compass,' a work which, incorporating also the 'Practical Rules,' placed within the reach of the educated seaman the theory and general principles of the magnetism of ships, as also so much of the elements of terrestrial magnetism as affected the navigator.

"In the Mercantile Marine, regulations for the examination and adjustment of the compasses are confined to sea-going passenger steamers. I gather from the letter of the Board of Trade, in reply to the Royal Society, as indeed I am aware from general personal knowledge, that practically, except perhaps in the larger shipping companies, these regulations are inoperative, or nearly so.

"For example, there are no prescribed rules as to the number, the position, or the efficiency of the compasses, and there is no guarantee for the competency of the adjuster, in whose hands the whole arrangements are generally placed. In many ports, and especially that of London, there is inefficient provision for swinging the ships.

"It appears unnecessary to remark, after what has just been briefly

stated, that the system adopted to ensure security of navigation in the Royal Navy has no counterpart in the Mercantile Marine. The assimilation in practice of the two services, so far as relates to the more essential points, would certainly be a desirable end to attain.

"I have already briefly detailed the two leading features of the Admiralty system:—The *first* of these (the navigating the ship by a standard compass) is in itself so simple, and has proved in practice so secure, and the neglect of it in many cases in merchant ships has been followed by such disastrous consequences, that I conceive there is no question that it should be enforced wherever there are the means of enforcement. Indeed, were it rendered imperative by law, that *every* vessel making a long sea voyage, and *every iron* vessel, whether employed coasting or foreign, should be fitted with a standard compass, I am of opinion this measure would not only directly tend to their secure navigation, but would indirectly tend to foster that knowledge of compass-laws and action now found to have become a necessity, when iron ships are the rule, and not the exception, as was the case some twenty years past. On the assumption that a measure of this nature must eventually obtain, I have appended a few short and simple rules (Appendix II.), which perhaps might be advantageously recommended by the authority of the Board of Trade, or Lloyd's Register Committee.

"With reference to the second leading feature of the Admiralty system:—

"For many years in the Royal Navy the adjustment practised consisted in the careful selection of a place for the standard compass, and the formation of a Table of Errors by the process of swinging the ship; and this proved sufficient so long as the deviations were moderate in amount.

"In many recent iron-built and iron-plated ships the amount of deviation is, however, so large that the employment of magnets to reduce the amount of deviation has become unavoidable; but the correction by magnets, however perfect it may be, is not considered in the Royal Navy as superseding the obtaining a Table of Errors and navigating the ship by that Table.

"The benefits which have been derived in the Royal Navy, both as regards the safety of ships, and the theoretical and practical knowledge of the subject we have thereby obtained, cannot, I think, be over-estimated; and I may add that I consider that no compass can be said to be 'properly adjusted' of which, whether compensated by magnets or not, a Table of Errors has not been obtained by the process of swinging the ship, and that Table examined by a competent person.

"Closely connected with the subject is that of the construction of the compass itself, as regards form and workmanship, magnetic power, and adjustment. This subject received much of the attention of the Committee I have referred to; and the result of their labours was the production of the 'Admiralty Standard Compass,' an instrument which has stood the test of twenty-five years' use, with little modification introduced, and

which has been adopted in all countries which directed their attention to this subject.

"Although indirectly the introduction of this compass into the Royal Navy has been the cause of much improvement in the compasses of the Mercantile Marine, there is still room for improvement. At present much expense is incurred in matters which are merely ornamental, and in some cases prejudicial. Probably much advantage would be derived from a model compass being fixed upon, which at a moderate price would supply the Mercantile Marine with the great desideratum of a compass of sufficient delicacy and accuracy. Considering that a few notes relating to the efficient points of a compass may prove useful, these notes will be found as Appendix III.

"There are yet two features in the 'Compass question' which appear to me as being worthy of consideration in any system that may be contemplated for assimilating the practice of the Mercantile Marine to that of the Royal Navy. These are,—

"1st. As to the efficiency of those who engage to perform the adjustments.

"2nd. The periods for examining the adjustments.

"By constant practice, but without any very clear knowledge of the principles of magnetism, several skilful adjusters of compasses are to be found at some of the great mercantile ports. These 'adjusters' must, from their practice, be now well known to the Board of Trade Surveyors. The registration of their names, and of the firms employing them, either by the local Marine Boards or by Lloyd's Committee, might be a desirable step to take as a preliminary measure.

"The arrangements for swinging ships, I have also heard, are either defective, or practically do not exist, at most of the mercantile ports; might not the Board of Trade Surveyors report upon the nature of existing arrangements, and the means generally adopted by the 'adjusters?'

"As to the periods for examining the adjustments, the recommendations of the Liverpool Compass Committee (see page 40, 3rd Report, 1861) appear to me to fully meet the case, and have such an important bearing on the secure navigation of iron ships, that I gladly bring them again to notice.

"'There appears sufficient reason for requiring that a new iron sailing ship or steamer should be swung immediately before each of the first two or three voyages; that all iron vessels should be swung immediately before the first voyage following any considerable amount of repair, whenever a change has been made in the position of the standard compass; when there is a change of Captain, unless the new Captain had charge of the vessel during the preceding voyage as Chief Officer.'

"In conclusion I must observe that the present state and prospects of the science and practice of the correction of the compass make it impossible to offer with confidence any complete set of suggestions as to the

system to be adopted in the Mercantile Marine. This could only be elaborated by careful and continued attention directed to the magnetic character of the ships of the Mercantile Marine, their compasses, and the capabilities of its officers; and I think it must be assumed that no system can be expected to be satisfactory which does not gradually develope itself under proper supervision.

"I have the honour to be, &c.

(Signed) "FREDERICK JOHN EVANS,
Staff-Commander R.N.,
Chief Naval Assistant, in charge of Magnetic Department."

"*The Hydrographer of the Admiralty.*"

APPENDIX I.

Extracted from the Queen's Regulations and the Admiralty Instructions for the government of Her Majesty's Naval Service.

"No iron of any kind is to be placed nor suffered to remain within the distance of seven feet of the binnacle or standard compasses, when it is practicable, according to the size and construction of the vessel, to remove it; and mixed metal or copper is to be substituted for iron in the bolts, keys, and dowels, in the scarphs of beams, coamings, and head-ledges, and also the hoops of the gaffs and booms and belaying-pins which come within the distance of seven feet of the compasses.

"The spindle and knees of the steering-wheels which come within the distance of seven feet of the compasses are also to be of mixed metal.

"Iron tillers, which work forward from the rudder-head, are not to range within seven feet of the compasses; and in vessels which have iron tillers working abaft the rudder-head, the binnacles are to be placed as far forward from the wheel as may be convenient for the helmsman to steer by.

"The boats' iron davits are to be placed as far as may be practicable and convenient from the compasses.

"All vertical iron stanchions, such as those for the support of the deck, or for the awnings, &c., and likewise the arm-stands, are to be kept beyond the distance of *fourteen feet* from the compasses in use, so far as the size of the vessel will admit.

"The binnacles for the steering-compasses are to be constructed upon a given plan, with tops made to take off; and in order to prevent improper materials from being deposited therein, they are not to be fitted with doors.

"For the better preservation of the compasses, in every ship a closet is to be constructed in a dry place, sufficiently large for the reception of the ship's establishment of compasses, and it is to be appropriated to that purpose *exclusively*, the key being kept by the Masters; and in order that

the spare compass-cards may never be kept with poles of the same name nearest to each other, cases are supplied which will prevent the possibility of their being packed improperly.

"All ships are to be swung before sailing from the port where they fit out, and subsequently once in each year, for the purpose of ascertaining the errors of the compasses, also immediately on their arrival on a Foreign Station; or if there has been any great change in the ship's geographical position since the errors were observed."

Appendix II.

Suggested Rules relating to the Compasses of Iron Merchant Ships.

"1. It is deemed a necessary equipment for every iron ship to be fitted with a Standard or navigating compass, in addition to one or more compasses for the use of the helmsman.

"2. That so far as the requirements of the ship will permit, special arrangements be made in the course of construction for preparing a place for this compass.

"3. That the Steering-Compasses being subordinate in importance to the Standard Compass, less strict precautions are required for their position; but it would in all cases be desirable that these compasses (and of necessity the steering-wheel) should not be placed within half the breadth of the ship from the stern-post, rudder-head, and screw-well.

"4. The Standard Compass to be placed at such a height from the deck (not less in any case than five feet) as to command a clear view of the horizon above the bulwarks, and to be out of the way of the sails, booms, &c.

"5. In ships built with their heads *near the north*, the Standard Compass to be placed as *far forward* as the requirements of the ship will permit. In ships built with their heads *near the south*, this compass to be placed as near the stern as convenient, subject to the condition that it should not be within half the breadth of the ship from the rudder-head, stern-post, or screw-well.

"In ships built near east and west, this compass should not be placed near either extreme of the ship.

"6. The Standard Compass to be as far as possible, and not less than ten feet, *from the end* of any elongated mass of iron, especially if vertical, such as iron stanchions, capstan-spindles, steam- and stove-funnels, ventilating-shafts, &c.; and no iron, subject to occasional removal, should be placed within fifteen feet of the Standard Compass, either on the same deck or below it.

"7. The Standard Compass to be placed as far as possible from transverse iron bulkheads.

"8. It would be an extremely desirable arrangement for the deck imme-

diately below the Standard Compass *not* to be of iron, but to be filled up with wood for a space which may be called the compass platform. This space should not be of less width than a hatchway (4 to 6 feet), and of as great length fore and aft as convenient, but the length not to be less than the width. No transverse iron deckbeams to be under the platform, but if necessary fore-and-aft iron stringers, on which the transverse beams outside the wooden surface may abut.

"9. It would be a desirable arrangement, as far as could be carried out, that no masses of iron, such as boilers, tanks, bulkheads, should be placed immediately below the compass, or within 55° of the vertical line through the centre (the angle being drawn from the compass as centre to the centre of the mass).

"10. Where the Standard Compass is placed on a bridge, the foregoing requirements should be, as far as possible, complied with, the bridge should be of wood, and should not have iron stanchions, or rails (especially if covered with brass) within 10 feet."

The following Rules are applicable to Steering-Compasses.

"1. Not to be within half the width of the ship from the stern-post, rudder-head, or screw-well.

"2. The spindle of the steering-wheel and the forward support in which it works, *not* to be of iron.

"3. Iron tillers working forward from the rudder-head not to range within six to seven feet of the steering-compass.

"4. Not to be near the upper (or lower) end of elongated masses of iron, especially if vertical, such as steam- and stove-funnels, capstan-spindles, &c., and to be as far as possible from any transverse iron bulkhead."

Special Points for the consideration of the Naval Architect.

"1. When arrangements are made for the compasses to be placed in the after part of the ship, building the vessel head north would ensure exaggerated errors both when upright and heeling.

"With building-slips in a meridional direction, and with the above arrangements, it would be desirable to build the ship head to the south.

"2. Every iron ship after launching, and during the process of first equipment, should as much as possible be kept in a position opposite to that she occupied on the building-slip."

Appendix III.

Notes relating to the efficient points of a Compass.

"1. The essential qualities of a good compass may be considered to embrace great sensibility and steadiness, with simplicity of construction. By

sensibility and steadiness it is to be understood that the needle is freely to submit to the earth's magnetic force, with power sufficient to steadily obey that force under the varying motions of a ship, without the aid of friction or mechanical impediment, steadiness or rather sluggishness produced by the latter causes being obtained at the expense of accuracy.

"Simplicity of construction, so that repairs can be effected by an ordinary skilled mechanic, must be deemed a qualification of merit.

"2. The chief points to be attended to in construction are,—

"(*a*) Great directive power of the needle, with little weight, and consequently little friction on the point of suspension.

"(*b*) Permanency of the magnetic power of the needle.

"(*c*) Accurate adjustment of the several parts of the compass. This comprises (1) the magnetic axis of the needle coinciding with the north and south points of the card. (2) The intersecting point of the axes of the jimbals of the bowl coinciding with the point of suspension of the card. (3) The accurate centering of the point of suspension within the bowl. (4) The perfect impression of the card, so that the centering and marginal divisions are not distorted by shrinking or other causes.

"3. The advantages of a compound system of needles compared with a single needle.

"These are, (1) greater directive power being obtained with the same weight. (2) The needles can be placed on their edge, whereby there can be no alteration of their magnetic axes, a condition frequently found in flat bar needles. (3) By placing one (or two) pairs of equal parallel needles with their ends 60° (or 30°) apart, the 'wabbling' motion common to single bar needles is avoided; and the following remarkable property also exists with this arrangement of the needles:—

"When magnets or soft iron are placed as correctors of the larger deviations due to the iron of the ship, unless the needle (where a single bar is employed) be very short compared to the distance of the disturbing magnet or iron, a deviation is introduced depending on the length of the needle. This deviation disappears with the compound arrangement.

"Proceeding from general principles to details, the following are the chief points to be attended to in the construction of a Standard Compass.

"1. The bowl to be constructed of *pure* copper, of substantial thickness, and the part adjacent to the needle increased in solidity, by an extra copper ring, the ends of the needle being permitted to work as close to the ring as consistent with freedom of motion.

"2. The needles to be fitted on the compound system (one pair to be deemed sufficient), and efficiently tempered and magnetized.

"3. The sight-vane to be arranged so as to turn freely in azimuth without moving the compass-bowl or causing disturbance to the card. It should be attached to a graduated circle, so as to show the angle between the ship's head and any celestial object as measured on the horizon without

using the compass-card. The sight-vane and graduated circle to be attached to the bowl.

"4. To be provided with one spare card, two spare caps, and four spare pivots.

"5. The caps to be fitted with rubies instead of agates. The pivots to be of steel hardened and tempered to a dark straw-colour."

Letter No. 1, from the President to Mr. Farrer, transmitting Memorandum.

"Burlington House, Nov. 2, 1865.

"SIR,—I have now laid before the Council of the Royal Society your letter of the 25th of July, referring to the adjustment of the compasses of iron ships, and a copy of my letter of the 28th of August, acknowledging its receipt and adverting to the inquiry you had made as to the preparation of a 'Manual' on the subject, together with your subsequent letter of October 23rd, having reference to the same inquiry.

"The President and Council are much disappointed by learning that the Board of Trade are not prepared to give effect to the recommendation that the system which has been found to work so successfully in the Royal Navy, of combining official and competent superintendence with a proper code of instruction, should be extended to the Mercantile Marine. They consider such superintendence to be essential, not only to the general introduction of a good and efficient mode of compass-correction into the Mercantile Marine, but even to the discharge of the duties having respect to the adjustment of the compasses of sea-going passenger-steamers with which the Board of Trade is already charged by the Legislature.

"In the Memorandum accompanying my letter of the 15th of May, it was stated that many recent losses of iron steamers have taken place in which it is probable that compass-errors have occasioned the loss. The President and Council think it right to call the attention of the Board of Trade to the serious responsibility they incur in cases of loss of life and property arising from the want of a proper system of compass-adjustment, by declining to take the course which is pointed out by the concurrent opinion of all competent advisers, as not only the best, but the only method of securing the introduction of such a system. They cannot but look forward to a time when the necessity of a proper supervision will be forced on the executive by public feeling, excited by some disastrous loss of human life traceable to the want of such superintendence. The question is one which they feel to be of such vital importance, that they desire to submit to the consideration of the Board of Trade the accompanying Memorandum, replying in some detail to passages in your letter of July 25th, and which makes it unnecessary to me to dwell further on the subject.

"I have the honour to be,

"Your obedient Servant,

"EDWARD SABINE,

"*President of the Royal Society.*"

Memorandum.

"The letter of the Secretary of the Marine Department of the Board of Trade of the 25th of July, to the President, conveying the views of the Board of Trade on the President's letter of the 25th of May, and the Memorandum which accompanied it, seem to require some detailed observations.

"To obviate the risk of misapprehension of the scope and object of the Memorandum, it appears advisable to state that the main object which the President and Council had in view, was not to suggest that the objects desired might be obtained by framing definite and positive rules and enforcing their observance by penalties, but primarily to show the importance of some superintendence of the adjustment of the compasses, of at least one important class of iron vessels, being entrusted to a department specially constituted for the purpose, and to point out some of the advantages which might be expected to flow directly and indirectly from such a department. The appointment of an officer, with proper assistants, for the purpose indicated, is not, it is apprehended, beyond the existing powers of the Board of Trade, and would not, it is conceived, violate any sound principle of political economy.

"The President and Council believe that, in considering the appointment of such an officer a matter of paramount importance, they are supported by the judgment of the persons most competent to form an independent opinion. They have in the former Memorandum referred to the opinion expressed by the Liverpool Compass Committee. Since that Memorandum was submitted to the Board of Trade, the Council have found that a similar opinion was expressed so long ago as the year 1839, by the Astronomer Royal, who then addressed to the Admiralty a Memorial of a formal character, of which one of the conclusions was,—

"'That it is expedient that the general superintendence of the compass in iron ships, for several years at least, be entrusted to some person appointed by the Government.'

"The Admiralty declined to appoint such an officer for the Mercantile Marine; but the very system recommended was introduced shortly afterwards into the Royal Navy, where experience has shown the very great advantages to be derived from it, and that in a service in which, if anywhere, obedience to positive rules without the intervention of a superintendent might have been supposed attainable. The Astronomer Royal has recently expressed his adherence to the opinion so expressed by him.

"The President and Council in the former Memorandum ventured to call attention to the duties in respect of the adjustment of the compasses of sea-going passenger-steamers, imposed by the Legislature on the Board of Trade, and to the imperfect mode in which those duties are at present discharged.

"The Board of Trade in its answer recognizes the importance of the subject, and admits that 'the present practice is far from satisfactory,' and that 'the steps taken by the Board of Trade under the provisions of existing Acts are not such as to remedy the evil;' but states that the Board see considerable difficulty in adopting all the suggestions made by the Royal Society.

"The difficulties are stated to be,—

"'1. That the powers under which the Board acts apply only to passenger-steamers, while the want which the Royal Society wish to meet is felt just as much in the case of other iron vessels, which are becoming more numerous every day.

"'2. That the powers of the Board of Trade only extend to obtaining a Certificate that the compasses have been properly adjusted. They do not enable the Board of Trade or its officers to see that the compasses are good, or to require, what the Royal Society appear to consider the most important condition of all, that there should be a Standard Compass (in addition to the Steering-Compass) so placed as to be free from local attraction.'

"With regard to the first of these difficulties, it cannot be necessary to suggest that the want of power as regards one class of vessels is no reason for not exercising the powers and discharging the duties of the Board as to another class of vessels. There are, however, other considerations which tend to show that it is not necessary to wait for extended powers. In the first place, on the establishment of a new department having new duties, there are some advantages in those duties being confined to a limited number of vessels. Again, all the indirect, and these not the least, advantages to be derived from such a department extend as much to vessels which do not come within the direct operation of the department as to those which do; and lastly, Shipowners and Underwriters, when the advantages of the department have been ascertained, may cause a voluntary submission of many vessels to the supervision of the Department.

"It is thus quite possible that experience may show that it is not necessary to obtain any legislative extension of the class of vessels to which the authority of the Board of Trade extends. If, on the other hand, it shall hereafter appear desirable to extend it, it is not to be anticipated that the Legislature will refuse to give extended powers.

"With regard to the second difficulty, it may be observed that the Board of Trade appear to put an unnecessarily restricted interpretation on the expression 'compasses properly adjusted' in the Merchant Shipping Act, 1854, Sec. 301.

"It is submitted with confidence that the expression in question enables and requires the Board of Trade and its Officers to see that one compass at least shall be in a position in which it is capable of being properly adjusted—a condition not generally consistent with its being the Steering-Compass—and therefore to require a special Certificate in the case of any Shipowner insisting on sending his ship to sea with only one com-

pass, or in which the navigating-compass does not fulfil the conditions prescribed. The information which the Council possess induces them to think that, under the present system, a large number even of sea-going passenger-steamers cannot be said to have their compasses 'properly adjusted'—and that owing to the causes pointed out in the 'Memorandum.' The President and Council do not apprehend that if the department recommended were established, its action would be impeded for want of authority.

"The President and Council therefore consider that even for the due discharge of the duties already imposed on the Board of Trade by the Legislature, some systematic superintendence on the part of the Board is necessary.

"With regard to the offer of the Board of Trade to communicate to Lloyd's Register Committee any practical rules which the Royal Society can furnish as to the supply, placing, and adjustment of compasses, and as to the effect on them of different modes of construction of the hull of the ship, the Board of Trade may be referred to the very valuable paper by Staff-Commander Evans, the Superintendent of the Compass Department of the Royal Navy, in answer to an application of the Board of Trade to the Admiralty, as containing everything which the President and Council could venture to suggest. The whole of this paper is well worthy of the most careful consideration; but there are some passages in it which bear so directly on the present subject, that they may be more specifically mentioned. In one of these Captain Evans states that the rule of navigating a ship by a standard compass is in itself so simple, has proved in practice so secure, and the neglect of it in many cases in merchant ships has been followed by such disastrous consequences, that he considers there is no question that it should be enforced, wherever there are the means of enforcement. In another passage Captain Evans states that he considers that no compass can be said to be 'properly adjusted,' of which, whether corrected by magnets or not, a table of errors has not been obtained by the process of swinging the ship, and that table examined by a competent person. In a third passage Captain Evans observes that the present state and prospects of the science and practice of the correction of the compass makes it impossible to offer with confidence any complete set of suggestions as to the system to be adopted in the Mercantile Marine; this could only be elaborated by careful and continued attention directed to the magnetic character of the ships of the Mercantile Marine, their compasses, and the capabilities of its officers; and that he thinks it must be assumed that no system can be expected to be satisfactory which does not gradually develope itself under proper supervision. They trust that the communication of this important paper to Lloyd's and its publication may be followed by beneficial results.

"The Board of Trade further say that, as regards the diffusion of information on the subject of compasses, especially among Merchant

Officers, the first desideratum appears to be a clear and intelligible manual, or set of directions on the subject containing such practical rules as the present state of Science can furnish, and such a statement of the principles as may be necessary for the comprehension of those rules; and inquire whether the Royal Society can put them in the way of obtaining such a manual, stating that any expense connected with its preparation will be readily defrayed by the Board of Trade.

"The President and Council do not consider the manual to be the *first* desideratum, but, on the contrary, they consider that, so long as the present system continues, such a manual would have a very limited and partial use. It will be remembered that in the Memorandum the Council itself suggested, as part of the general scheme proposed, that notice might be given that after a certain period, say two or three years, a certain amount of knowledge will be required from Candidates, and that in the meantime a text-book containing the necessary amount of information might be prepared and published; and they conceive it would be one of the earliest duties of the proposed department to cause such a text-book to be prepared; but the President and Council conceive that it would be premature to prepare it until the system to be pursued has been decided on, and without the concurrence of the person to be charged with carrying it into effect.

"As regards introducing the subject of the deviation of the compass into Examinations in Navigation, the President and Council will be happy to give any information or assistance in their power. They feel, however, as in the case of the text-book they have referred to, that such examination should follow, not precede the appointment of a Superintendent, and should be under his direction.

"As regards inquiries into the causes of wrecks, the Council are happy to find that the Board of Trade are disposed to take some step in the direction indicated in the Memorandum.

"In the former Memorandum attention was called to the importance, as regards the advancement of the science of the deviation of the compass, of observations of the deviations of the same compass in the same ship at different times and places being made and systematically reduced and discussed. Trustworthy observations of this kind are now among the principal desiderata in this science. As regards such observations, the Board of Trade state that all they can do is to obtain observations from Masters of Merchant ships in the manner originally proposed by the Royal Society when the Meteorological Department of that office was established, and that the subject will come under the consideration of the Board, with the whole subject of the Meteorological Department.

"The proposal made by the Royal Society in the year 1855, in con-

nexion with the Meteorological Department, had reference to Terrestrial Magnetism, not to the deviations of Iron Ships; and they would observe, as regards any observations of such deviations, that the whole scientific value of such observations depends on their being made in strict conformity with corresponding observations made in the same vessel, and under the same precise conditions at home. No such conformity can be expected or ensured unless with some system of supervision. It may be further observed that the value of such observations depends on the compass by which the observations are made being one fulfilling the conditions recommended with reference to the navigating-compass. For the Meteorological Department to obtain and deal with such observations it would be necessary that it should possess an Officer qualified to discharge, and discharging, many of the duties of such a Superintendent as is recommended by the Council. Finally it may be observed that Shipmasters cannot be expected to make or transmit such observations, unless encouraged so to do, by knowing that the observations when made have a real value, and that they will be appreciated, made use of, and publicly acknowledged."

Letter No. 2, from the President to Mr. Farrer.

"Burlington House, Nov. 2, 1865.

"SIR,—I have laid before the Council of the Royal Society your Letter of the 24th of October, in reference to the Meteorological Department, and am authorized to make the following reply:—

"The President and Council fully concur with the Board of Trade regarding the importance of inquiries being made into the value of the observations obtained at sea under the direction and guidance of the Meteorological Department of the Board of Trade, and into the steps which should be taken to utilize the results, as well as the importance of the further question, whether any, and, if any, what future observations of the same or of a similar kind bearing on Ocean Statistics should be collected. They will be quite ready to assist in this inquiry in the manner proposed, viz. by nominating one of their Fellows conversant with such subjects, as a member of the proposed Committee *.

"In reference to the last paragraph of your letter of the 24th October, they are of opinion that systematic meteorological observations at a few selected land stations in the British Islands are desirable, in addition to the meteorological observations at sea, in order to complete a suitable contribution from this country to the meteorological investigations now in progress in the principal States of Europe and America, under the authority of their respective Governments.

"If, in the communication from the Royal Society to the Board of Trade of February 22, 1855, which preceded the establishment of Admira

* [The Council nominated Mr. Francis Galton, F.R.S., to serve on the Committee referred to

FitzRoy's Office, the advantages to be derived from a continued and well-directed system of maritime observations were more particularly pressed, it was because at that time neither the instruments nor the modes of observation suitable for a well organized and efficient system of continuous land investigation were prepared. This was well stated by Lieut. Maury in a letter addressed to the United States Government, dated November 6, 1852, subsequently transmitted by that Government to the Earl of Clarendon, and printed in the papers which were presented to the House of Lords in February 1853. This difficulty no longer exists, having been entirely obviated by the self-recording system of observation for which the necessary instruments have been devised and brought into use at the Kew Observatory.

"The President and Council are not aware of any inconvenience likely to arise from entrusting the scientific supervision of such a system as they have recommended to a Body such as the Kew Committee, acting under the authorization, and control in regard to expenditure, of a Public Department. Precedents for such a course are not wanting.

"I have the honour to be

"Your obedient Servant,

"EDWARD SABINE,

"*President of the Royal Society.*"

From Mr. Farrer to General Sabine.

"Board of Trade, Whitehall, 14th November, 1865.

"SIR,—I am directed by the Board of Trade to acknowledge the receipt of your letter of the 2nd instant, stating that the President and Council of the Royal Society 'are much disappointed by learning that the Board of Trade are not prepared to give effect to the recommendation that the system which has been found to work so successfully in the Royal Navy, of combining official and competent superintendence with a proper code of instructions, should be extended to the Mercantile Marine. They consider such superintendence to be essential, not only to the general introduction of a good and efficient mode of compass-correction into the Mercantile Marine, but even to the discharge of the duties having respect to the adjustment of the compasses of sea-going passenger-steamers, with which the Board of Trade is already charged by the Legislature.'

"The President and Council further proceed to call attention to the losses of Iron Steamers, and intimate that the responsibility for such losses will rest with this Board if they do not undertake the superintendence of compasses in the mode suggested by the Royal Society.

"In reply I am to state to you, in the first place, that the Board of Trade do not yield to the President and Council of the Royal Society in their anxiety to prevent losses at sea, and they are ready with this object to do everything which is within the proper and legitimate scope of their functions as a Government department.

"What the scope of those functions is, and how they can be most usefully exercised, are questions on which they must form their own opinion, and they regret that the opinion they have thus formed is at variance with the views which the President and Council of the Royal Society have thought fit to urge.

"As regards the practice of the Admiralty to which you call attention, I am to point out in the first place, that there is a wide difference between the relation of the Board of Admiralty to Her Majesty's Navy, and that of the Board of Trade to the Mercantile Marine. This difference appears to have been underrated, if not entirely overlooked by the President and Council of the Royal Society.

"The Admiralty are the owners, designers, and generally the builders of the Ships of the Nation, and in these capacities are bound to use every means in their power to construct the National Ships in the best manner, to provide them with the best equipments, and to dictate and enforce, upon all persons concerned in building, equipping or navigating them, such arrangements and regulations as the most advanced science and the latest experience can suggest. On the other hand, the Board of Trade are not the owners, designers, or builders of Merchant Ships; and if they were to take upon themselves the responsibility of regulating the construction of every Merchant Ship, and of requiring her to be provided with what might appear to this Board to be necessary and proper equipments, they would be usurping a power they do not possess, and which as a matter of policy they ought not to possess. They would in so doing be taking upon themselves a function which belongs to the shipowner, and which it is his interest, as well as his duty, to perform efficiently. It can be no part of the functions of Government to put a stop to the free and healthy action of that self-interest, or to relieve the shipowner and his servants from his responsibility for the performance of that duty.

"The result thus arising from Government interference would, the Board of Trade are satisfied, be injurious to trade in the first instance, whilst it would in the end be no less prejudicial to the safety of the public, and to the advancement of science.

"But if, looking to certain precedents, the President and Council of the Royal Society should still urge that in the special and exceptional case of deviation of ship's compasses it is the duty of the Government to depart from the principles generally admitted in this country, the Board of Trade would reply that, so far as they can judge, the subject of compass-deviation is one which in its present condition is peculiarly unfit for legislative or administrative interference. Where a precautionary measure is capable of being reduced to fixed, simple, and intelligible rules of practice, it is possible, even though it may not be advisable, to enforce it by legal and administrative process. But this subject is, so far as the Board of Trade can judge, far from being in that condition.

"It appears from the papers submitted to the Board of Trade in this

case, that the causes of deviation of the compass in each individual ship are numerous and dissimilar, and their effects proportionately varied. In addition to the variety of effects due to the variety of causes, these effects seem also to vary according to the build of the ship, the nature and quality of the material of which she is built, and the direction of the line of the keel during building, the nature, quantity, and stowage of the cargo, the ship's course for the time being, her position in the water for the time being, the magnetic hemisphere in which she may be, and the varying distance of the ship from the magnetic equator. They vary, too, it would seem, from time to time, according to the service on which the ship may be or may have been employed, and with the age of the ship. Science has undoubtedly done much to ascertain the laws that govern these numerous causes of error; but it is obvious, even from the tentative and experimental process which the President and Council of the Royal Society themselves suggest, and from the difficulty they find in preparing the specific directions for which the Board of Trade have asked, that the remedy is not capable of being reduced to fixed or simple rules, or of being enforced without a large and experienced staff of scientific officers, or without an amount of minute arbitrary and indeterminate supervision which would be intolerable and impracticable. Moreover, so far as the Board of Trade can learn, the highest authorities are not yet agreed as to the principle of the remedy, the practice of the Admiralty, which receives the approval of the Royal Society, being founded in the main on one principle, whilst the practice of the Mercantile Marine is founded on another and different principle, which is supported by no less an authority than the Astronomer Royal.

"In a letter from the Admiralty to this Board, dated 14th September last, are enclosed some memoranda by Commander Evans, R.N., of the Compass Department. These memoranda the Royal Society indorse in the printed memorandum enclosed in their last letter. In them it is stated that the principal features of the system followed in Her Majesty's Navy are, '1. By having in each ship a standard compass distinct from the steering-compass * * * by which compass alone the ship is navigated,' and '2. The requiring each ship to be swung and to be navigated by a Table of Errors.'

"On the other hand, the Astronomer Royal, in his syllabus of a course of lectures delivered this year to the Royal School of Naval Architecture and Marine Engineering, states that he 'has no hesitation in giving his own opinion that the compasses used for directing the ship's course ought to be corrected, and that the efforts of scientific men ought to be directed mainly to the rendering this correction rigorously accurate and easy of application.'

"The Board of Trade have, as the President and Council of the Royal Society are aware, already published and circulated Mr. Towson's work, a work 'strongly recommended to nautical men' by the Astronomer Royal,

and approved by the Assistant Hydrographer; they are, as the Royal Society are also aware, prepared to print and circulate amongst all persons interested any practical hints or directions that the President and Council of the Royal Society, the Admiralty, or the Astronomer Royal may be able to furnish; and they are also prepared to procure the best scientific help upon investigation into wrecks in any case in which it may appear that a wreck may have been caused by compass-errors.

"But the Board of Trade, for the reason above stated, are not prepared to assume the responsibility which would be involved in appointing an officer or officers whose duty it should be to superintend the compasses of Merchant Ships, and to enforce upon shipowners and navigators compliance with what such officers may believe to be the latest requirements of science.

"In coming to this conclusion, the Board of Trade believe that they are doing what is most calculated to promote the free and healthy development of scientific results as applied to the Mercantile Marine, as well as to further what are their own proper objects, viz. the benefit of trade and the public safety.

"I have the honour to be, Sir,
"Your obedient Servant,
"T. H. Farrer."

"*The President of the Royal Society,*
Burlington House, Piccadilly."

[Pursuant to instruction, the Secretary acknowledged the receptionf the above letter by the President and Council.]

From Sir J. Emerson Tennent to General Sabine.

"Office of Committee of Privy Council for Trade,
Whitehall, 20th November, 1865.

"Sir,—With reference to your letter of the 2nd November, stating the willingness of the President and Council of the Royal Society to appoint one of their Fellows to represent the Society upon a Committee to examine and report on questions connected with the Meteorological Department of the Board of Trade, I am directed by the Lords of the Committee of Privy Council for Trade to inform you that Staff-Commander Evans has been nominated by the Admiralty, and Mr. Farrer by this Board; and I am at the same time to request you to be good enough to forward the name of the gentleman selected by the President and Council of the Royal Society.

"The following are the points which the Board of Trade propose to refer to the Committee, if the President and Council see no objection.

"1. What are the data, especially as regards Meteorological Observations made at Sea, already collected by and now existing in the Meteorological Department of the Board of Trade?

"2. Whether any, and what steps should be taken for arranging, tabulating, publishing, or otherwise making use of such data.

"3. Whether it is desirable to continue Meteorological Observations at Sea; and if so, to what extent, and in what manner.

"4. Assuming that the system of Weather Telegraphy is to be continued, can the mode of carrying it on and of publishing the results be improved?

"5. What Staff will be necessary for the above purposes?

"I have the honour to be, Sir,

"Your obedient Servant,

"J. Emerson Tennent."

"*The President of the Royal Society.*"

[The President replied to this letter, and forwarded the name of Mr. Francis Galton, F.R.S., selected by the Council to be a Member of the Committee.]

December 7, 1865.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

It was announced from the Chair that the President had appointed the following Members of the Council to be Vice-Presidents:—

The Treasurer.
Mr. Gassiot.
Sir Henry Holland.

Mr. Alfred Tennyson, Poet Laureate, and Mr. Robert Grant, were admitted into the Society.

The following communications were read:—

I. "Addition to the Memoir on Tschirnhausen's Transformation." By Arthur Cayley, F.R.S. Received October 24, 1865.

(Abstract.)

In the memoir "On Tschirnhausen's Transformation," Phil. Trans. vol. clii. (1862) pp. 561–568, I considered the case of a quartic equation: viz. it was shown that the equation

$$(a, b, c, d, e \between x, 1)^4=0$$

is, by the substitution

$$y=(ax+b)\mathrm{B}+(ax^2+4bx+3c)\mathrm{C}+(ax^3+4bx^2+6cx+3d)\mathrm{D},$$

transformed into

$$(1, 0, \mathfrak{C}, \mathfrak{D}, \mathfrak{E} \between y, 1)^4=0,$$

where $(\mathfrak{C}, \mathfrak{D}, \mathfrak{E})$ have certain given values. It was further remarked that $(\mathfrak{C}, \mathfrak{D}, \mathfrak{E})$ were expressible in terms of $\mathrm{U}', \mathrm{H}', \Phi'$, invariants of the two forms $(a, b, c, d, e \between \mathrm{X}, \mathrm{Y})^4$, $(\mathrm{B}, \mathrm{C}, \mathrm{D} \between \mathrm{Y}, -\mathrm{X})^2$, of I, J, the inva-

riants of the first, and of Θ', $=BD-C^2$, the invariant of the second of these two forms,—viz. that we have

$$\mathfrak{C}=6H'-2I\Theta',$$
$$\mathfrak{D}=4\Phi',$$
$$\mathfrak{E}=IU'^3-3H'^2+I^2\Theta'^2+12J'\Theta'U'+2I'\Theta'H'.$$

And by means of these I obtained an expression for the quadrinvariant of the form $(1,\ 0,\ \mathfrak{C},\ \mathfrak{D},\ \mathfrak{E}\between y,\ 1)^4$; viz. this was found to be

$$=IU'^2+\tfrac{4}{3}I^2\Theta'^2+12J\Theta'U'.$$

But I did not obtain an expression for the cubinvariant of the same function: such expression, it was remarked, would contain the square of the invariant Φ'; it was probable that there existed an identical equation, $JU'^3-IU'^2H'+4H'^3+M\Theta'=-\Phi'^2$, which would serve to express Φ'^2 in terms of the other invariant; but, assuming that such an equation existed, the form of the factor M remained to be ascertained; and until this was done, the expression for the cubinvariant could not be obtained in its most simple form. I have recently verified the existence of the identical equation just referred to, and have obtained the expression for the factor M; and with the assistance of this identical equation I have obtained the expression for the cubinvariant of the form $(1,\ 0,\ \mathfrak{C},\ \mathfrak{D},\ \mathfrak{E}\between y,\ 1)^4$. The expression for the quadrinvariant was, as already mentioned, given in the former memoir: I find that the two invariants are in fact the invariants of a certain linear function of U, H; viz. the linear function is $=U'U+\frac{2}{3}\Theta'H$; so that, denoting by I*, J* the quadrinvariant and the cubinvariant respectively of the form $(1,\ 0,\ \mathfrak{C},\ \mathfrak{D},\ \mathfrak{E}\between y\quad)^4$, we have

$$I^*=\tilde{I}(U'U+4\Theta'H).$$
$$J^*=\tilde{J}(U'U+4\Theta'H),$$

where $\tilde{I}$, $\tilde{J}$ signify the functional operations of forming the two invariants respectively. The function $(1,\ 0,\ \mathfrak{C},\ \mathfrak{D},\ \mathfrak{E}\between y,\ 1)^4$, obtained by the application of Tschirnhausen's transformation to the equation $(a,\ b,\ c,\ d,\ e\between x;\ 1)^4=0$, has thus the ***same invariants*** with the function

$$U'U+4\Theta'H$$
$$=U'(a,\ b,\ c,\ d,\ e\between x,\ 1)^4+4\Theta'(ac-b^2,\ ad-bc,\ ae+2bd-3c^2,\ be-cd,\ ce-d^2\between x,\ 1)^4,$$

and it is consequently a linear transformation of the last-mentioned function; so that the application of Tschirnhausen's transformation to the equation $U=0$ gives an equation linearly transformable into, and thus virtually equivalent to, the equation $U'U+4\Theta'H=0$, which is an equation involving the single parameter $\dfrac{4\Theta'}{U'}$: this appears to me a result of considerable interest. It is to be remarked that Tschirnhausen's transformation, wherein y is put equal to a rational and integral function of the order

$n-1$ (if n be the order of the equation in x), is not really more general than the transformation wherein y is put equal to any rational function $\frac{\mathrm{V}}{\mathrm{W}}$ whatever of x; such rational function may, in fact, by means of the given equation in x, be reduced to a rational and integral function of the order $n-1$; hence in the present case, taking V, W to be respectively of the order $n-1$, $=3$, it follows that the equation in y obtained by the elimination of x from the equations

$$\mathrm{V}=(a, b, c, d, e \between x, 1)^4=0,$$

$$y=\frac{(\alpha, \beta, \gamma, \delta \between x, 1)^3}{(\alpha', \beta', \gamma', \delta' \between x, 1)^3}$$

is a mere linear transformation of the equation $\mathrm{AU}+\mathrm{BH}=0$, where A, B are functions (not as yet calculated) of $(a, b, c, d, e, \alpha, \beta, \gamma, \delta, \alpha', \beta', \gamma', \delta')^4$.

II. "A Supplementary Memoir on the Theory of Matrices." By Arthur Cayley, F.R.S. Received October 24, 1865.

(Abstract.)

M. Hermite, in a paper "Sur la théorie de la transformation des fonctions Abéliennes," Comptes Rendus, t. xl. (1855), establishes incidentally the properties of the matrix for the automorphic linear transformation of the bipartite quadric function $xw'+yz'-zy'-wx'$, or transformation of this function into one of the like form, $\mathrm{XW}'+\mathrm{YZ}'-\mathrm{ZY}'-\mathrm{WX}'$. These properties are (as will be shown) deducible from a general formula in my "Memoir on the Automorphic Linear Transformation of a Bipartite Quadric Function," Phil. Trans. t. cxlviii. (1858), pp. 39–46; but the particular case in question is an extremely interesting one, the theory whereof is worthy of an independent investigation. For convenience the number of variables is taken to be *four*; but it will be at once seen that as well the demonstrations as the results are in fact applicable to any *even* number whatever of variables.

III. "On the Existence of Glycogen in the Tissues of certain Entozoa." By Michael Foster, M.B. Communicated by Professor Huxley. Received November 4, 1865.

Although glycogen has been found by various observers in the tissues of many of the Invertebrata, no one, as far as I know, has noticed the very remarkable amount which may be obtained from some of the Entozoa. I first came across this fact while working upon a tape-worm; unfortunately I neglected to determine the quantity of glycogen I obtained, and have not since had an opportunity of repeating the observation. The following remarks apply only to the round worm (*Ascaris lumbricoides?*) which dwells in the intestines of the common pig.

By mincing and boiling in water, with a drop of dilute acetic acid, one of these animals, a decoction is obtained which remains milky-looking and opalescent after several filtrations, and therefore at once suggests the idea of glycogen. This milky fluid strikes a deep port-wine red with iodine, the colour disappearing on the application of heat, and reappearing on cooling, and gives no reduction when boiled with the alkaline copper-solution. When treated with saliva at 35° C. the opalescence disappears, leaving a fluid either perfectly clear or exhibiting only a few flakes or a slight cloudy deposit (of some albuminoid material), but containing much sugar, as may be shown both by the copper and fermentation test.

If the original milky fluid be thrown into spirit, an abundant white flaky precipitate is thrown down, consisting partly of some albuminoid substance, but chiefly of a substance giving all the above reactions of glycogen. If the fluid be thrown into glacial acetic acid, a white flaky precipitate is thrown down consisting of nearly pure glycogen. The presence of glycogen may also be shown by employing the alcoholic solution of potash. From these facts we may infer that glycogen, and not dextrine merely, does exist in the bodies of these animals.

In no case have I found this glycogen to be accompanied by anything more than a mere doubtful trace of sugar—that is to say, a trace of some substance giving a doubtful reduction of the copper-solution, and that by no means always. Hence, seeing how difficult it is to obtain glycogen in so pure a state that its quantity may be estimated directly by weighing, I have contented myself with determining the amount present in these animals by exposing a decoction to the influence of saliva until all traces of glycogen were lost, and then estimating by the copper process the amount of sugar produced. In this way I obtained from two ascarides weighing together, when taken fresh from the pig and merely wiped, 10·2 grms., and from three weighing together 10 grms., just 2·2 per cent. of sugar (on the wet weight) in each case. When this amount is compared with that produced by the mammalian liver alone, it will be seen that it really is, comparatively speaking, excessive. For the sake of comparing the Ascaris with other invertebrata, I may say that in a caterpillar weighing about 6 grms. I obtained a hardly appreciable quantity of glycogen, which was contained partly in the muscular parietes, and partly in the so-called "epiploon" or "hepatic parenchyma." The quantity of glycogen that I obtained from a handful of common maggots was also hardly appreciable.

In the Ascaris little or no glycogen is to be found in the intestine, a small quantity in the generative apparatus, and a very considerable quantity in the spongy visceral tissue; by far the largest amount exists in the firmer muscular parietes. I failed to detect with iodine any distinct histological localization.

It seems singular that an animal, living in the midst of a fluid one of whose chief functions is to change starch into sugar, should thus be found amassing glycogen within its own body. I have satisfied myself, however,

that there is no sugar-forming ferment present in the tissues of the Ascaris. Portions of the tissues may be kept exposed to a temperature of 35° C. for many hours without any appreciable loss of glycogen or advent of sugar. The whole animal, too, may be kept for days without any change of its glycogen being observed. I also failed to extract from the tissues any ferment capable of acting upon starch. The intestine alone seemed to have any power of the kind, and that but in the very feeblest degree. This failure in the production of sugar is not due to the presence of any substance antagonistic to the action of a sugar-forming ferment; for the addition of a small quantity of saliva to even the unboiled tissues very speedily brings about the conversion of the glycogen into sugar. We may infer therefore that, if the animal swallows the intestinal juices in which it lives, the sugar-forming ferment contained therein either does not pass through its intestinal wall into the visceral cavity, or, if it does pass, is at once destroyed. It is evident that the formation of glycogen in the Ascaris takes place under conditions very different from those under which glycogen is deposited in the mammalian liver, since in the latter case there is present a powerful sugar-forming ferment belonging, as we have reason to believe, to the liver-substance itself, and not merely to the blood passing through the organ.

The possible use of this glycogen is a matter of interest. Intestinal worms, inasmuch as they are animals and live, must needs consume oxygen. The amount of that gas they find in the intestinal juices, however, is very small; and, having a constant temperature secured to them by warmth external to themselves, they are the very last of creatures to need what has been called "respiratory or calorifacient material." Whatever be the use of sugar, starch, or glycogen in the mammalian body, no "respiratory" use can be safely suggested for the large amount of glycogen occurring in the Ascaris. Its abundance in the muscular parietes might suggest that it was material on its way to become muscle. If so, since the animals I studied were adults and ova-producing, the analogy of their glycogen would be, not with the glycogen of the muscles of the early mammalian embryo, but with the glycogen (or dextrine) occurring in smaller quantities in the full-grown muscles (unless one were to push an idea, and say that the tissues of the lower animals were *chemically* homologous with the embryonic tissues of the higher ones).

It might be thought to be immature chitin; but why should it exist in such quantities? and why is there so little in the caterpillar and the maggot? In the Tænia the glycogen could hardly be thought to have a muscular future. There it might be considered to be stored up for the development of the ova. This idea is at first sight contradicted, as far as the Ascaris is concerned, by the fact that very little glycogen can be obtained from the generative apparatus of that animal. But is it not possible that, though stored up elsewhere, it may really be intended for the ova and embryos after all? The analogy between the Ascaris, with its

glycogen, and a plant with its blanched starch-storing tissues, is striking in many ways. May not "migration," which plays so important a part in vegetable physiology, occur in the animal economy in reference to other substances besides fat?

IV. "On the Development of certain Infusoria." By J. SAMUELSON, Esq. Communicated by WILLIAM CROOKES, Esq. Received November 8, 1865.

(Abstract.)

The chief object of this paper is to account in some degree for the successive appearance, in organic infusions, of what seem to be distinct species of Protozoa rising in the developmental scale; but the author commences with some general remarks on the origin of these animalcules, nnd states, among other conclusions at which he has arrived, his disbelief in spontaneous generation as it is understood by Pouchet and his disciples.

Proceeding to the immediate purpose of the paper, the author first refers to the well-known fact, that when an infusion of decaying organic matter is exposed to the air, the types of Protozoa which first appear in it are the so-called Monads, and occasionally the particles of organized protoplasm known as *Amœbæ*, but that these in a few days in great part disappear, and are succeeded by ciliated infusoria, such as *Kolpoda, Cyclidium, Glaucoma*, and sometimes *Vorticella*—oftentimes followed in their turn by other types, as *Oxytrichum, Euplotes, Kerona*, &c. This phenomenon, he remarks, has been accounted for in different ways; but his own observations and experiments justify, in his opinion, the conclusion that the monadine form which first appears is the earlier or larval stage of at least one, if not more of the ciliated infusoria, into which it becomes metamorphosed in the progress of development. In the first place, he states that he and Dr. Balbiani have observed the regular occurrence of monads belonging to the species of *Cercomonas fusiformis* or *acuminata* of Dujardin, in pure distilled water which has been exposed some time to the atmosphere. These, or their zoospores, would seem to be wafted by the air along with particles of dust to which they cling. They readily appear when dust is sifted into distilled water, and in this way have been obtained from different localities at home, and, along with other forms, in dust shaken from rags imported from various distant parts of the world. An experiment is then related, conducted by the author during the hot weather of last summer, in which a comparison was made between the animalcules which made their appearance in a vessel of pure distilled water exposed to the air, and those successively appearing in distilled water to which extract of lettuce had been added. In both liquids *Cercomonades* speedily showed themselves; but whilst they remained unchanged in form in the pure water till near the end of the experiment (a period of about three weeks), they entirely disappeared from the lettuce-infusion in six or seven days, and were suc-

ceeded by ciliated infusoria. The fusiform body of the *Cercomonas* bears a long whip-like cilium at its anterior end, and a short hair-like caudal process at the opposite extremity. Now this characteristic figure was retained by the monads in the distilled water; they continued to grow larger during the progress of the observations, but without change of form; only, towards the end, some of them lost their caudal process, and fixed themselves by their anterior cilium, and others, retaining both appendages, became fixed by the caudal one as on a pedicle; finally, on exposure to undue heat and light, they shrank up, and then sometimes their soft substance was ejected from its enclosure and assumed the aspect and characters of an *Amœba*. On the other hand, the *Cercomonades* of the lettuce-infusion in a few days lost both appendages, and, changing their manner of swimming, began to move through the water like ordinary ciliated infusoria. Moreover a few days later these animalcules, on being fed with indigo, readily ingested it, whereas, although that substance was supplied freely to the *Cercomonades*, it was never observed within their bodies. Figures to illustrate these phenomena accompany the paper.

From these observations, the author infers that the *Cercomonades* are larvæ or earlier forms of the ciliated animalcules which succeed them; and he concludes his paper by remarking that, whilst he has confidence in the general accuracy of his observations, and in the views deduced from them, nevertheless, seeing the difficulties which attend such observations, and their consequent liability to error, he should be pleased were the same experiments repeated by others, in order to the confirmation or, if need be, the correction of his statements.

December 14, 1865.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "Numerical Elements of Indian Meteorology.—Series III. Temperatures of the Atmosphere, and Isothermal Profiles of High Asia." By HERMANN DE SCHLAGINTWEIT, Sakünlünski, Ph.D., LL.D., Corr. Memb. Acad. Leop.-Carol., &c. Communicated by Lieut.-General SABINE, P.R.S. Received August 21, 1865.

The principal object of this paper was to trace the relation between the decrement of mean temperature and the increment of height above the level of the sea in different regions of High Asia, to connect the variations observed from the general mean of the whole (390 feet increase of height for a diminution of 1° F. in mean temperature) with the variations

of season and of geographical position, and also to point out the cause of certain peculiarities in the climate of High Asia.

As it has been ascertained that the paper was read to the Academy of Sciences at Berlin on the 1st of June, and is printed at length, with its accompanying plates, in the 'Monatsbericht' for August 1865, the reader is referred to that puhlication.

II. "On testing Chronometers for the Mercantile Marine." By John Hartnup, F.R.A.S., Director of the Liverpool Observatory. Communicated by the President. Received November 22, 1865.

The late Admiral Beechey, on looking over the records of the Liverpool Observatory in 1854, was strongly impressed with the importance of some systematic plan being adopted for testing the chronometers employed in the Mercantile Marine. He consulted many persons on the subject who were interested in the security of navigation, but the difficulty which presented itself at that time was the long period required for the test, five or six months at least being supposed to be necessary.

About four years ago the Mersey Docks and Harbour Board gave me permission to have constructed, for the purpose of testing chronometers, a hot-air apparatus on a more convenient principle, and on a much larger scale, than the one which I had heretofore employed; and the arrangements are now so perfect that chronometers can be tested efficiently in five weeks. It appears that chronometers in the Merchant service, when at sea, are generally exposed to temperatures ranging from about 55° to 85° of Fahrenheit, and that for most practical purposes it is sufficient for the shipmaster to know the rate in the three definite temperatures 55°, 70°, and 85°. The following examples, taken from our records, will illustrate the method I have devised to supply this information. The temperature is changed 15° on Saturday mornings. No comparisons being made on Sundays, the rate for Monday in each week is the mean of two days.

Table I.—Showing the daily rates, gaining, of six chronometers for five weeks ending February 21.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	Mean daily temp.
	s	s	s	s	s	s	°
January 19	0·5	0·6	3·4	2·8	1·3	2·5	55
„ 20	0·6	0·7	3·5	3·1	1·1	2·9	55
„ 21	0·9	0·5	3·6	3·0	1·0	2·9	55
„ 22	0·9	1·0	3·5	3·1	1·3	2·5	56
„ 23	0·5	0·9	3·5	3·1	1·4	2·2	55
„ 24	0·6	0·8	3·6	3·0	1·1	2·3	55
Means	0·67	0·75	3·52	3·02	1·20	2·55	55

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	Mean daily temp.
	s	s	s	s	s	s	°
January 26	1·2	1·1	1·8	2·1	3·3	2·1	70
„ 27	1·2	1·4	1·8	2·0	3·3	2·2	70
„ 28	1·2	1·5	1·9	2·3	3·3	2·5	70
„ 29	1·2	1·6	2·1	2·3	3·1	2·3	70
„ 30	1·0	1·7	1·9	2·3	3·1	2·5	70
„ 31	0·9	1·4	2·0	2·2	3·4	2·7	71
Means	1·12	1·45	1·92	2·20	3·25	2·38	70
February 2	0·8	0·7	0·8	0·4	4·3	4·4	85
„ 3	0·7	0·8	0·9	0·5	4·3	4·8	85
„ 4	0·7	0·6	0·8	0·6	4·1	4·6	84
„ 5	0·4	0·7	0·7	0·3	4·0	4·3	85
„ 6	0·6	0·6	0·7	0·5	3·9	4·3	85
„ 7	0·9	0·9	0·9	0·6	3·9	4·3	85
Means	0·68	0·72	0·80	0·48	4·08	4·45	85
February 9	1·1	1·2	2·1	2·3	2·8	2·2	70
„ 10	1·3	1·3	1·7	2·3	2·9	2·1	70
„ 11	1·6	1·4	1·7	2·4	3·0	2·2	70
„ 12	1·4	1·6	2·2	2·6	2·8	2·3	71
„ 13	1·5	1·2	2·1	2·3	2·4	1·7	69
„ 14	1·5	1·2	2·2	2·4	2·4	1·8	69
Means	1·40	1·32	2·00	2·38	2·72	2·05	70
February 16	1·0	0·6	3·6	3·4	0·5	2·8	55
„ 17	0·9	0·4	3·9	3·3	0·3	2·5	55
„ 18	0·9	0·8	3·9	3·5	0·4	2·3	55
„ 19	0·6	0·4	3·6	3·3	0·4	2·1	55
„ 20	0·8	0·7	3·4	3·6	0·3	2·3	56
„ 21	0·7	0·9	3·9	3·6	0·7	2·6	56
Means	0·82	0·63	3·72	3·45	0·43	2·43	55

From these six examples, the following results for the middle period of the test are deduced:—

TABLE II.—Showing the mean daily rates, gaining, in three definite temperatures.

Mean temperature.	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
°	s	s	s	s	s	s
55	0·75	0·69	3·62	3·24	0·82	2·49
70	1·26	1·39	1·96	2·29	2·99	2·22
85	0·68	0·72	0·80	0·48	4·08	4·45

TABLE III.—Showing the weekly increase of gaining-rate deduced from the first and last weeks of the test.

No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
s	s	s	s	s	s
0·04	−0·03	0·05	0·11	−0·19	−0·03

The efficiency of the method will be seen by the following three examples, in which the test was repeated four times in succession.

Table IV.—Showing the mean daily rates, gaining, of three chronometers tested in three definite temperatures four times in succession.

Middle period of test.	No. 1. 55°	70°	85°	No. 2. 55°	70°	85°	No. 3. 55°	70°	85°
	s	s	s	s	s	s	s	s	s
November 12	2·4	2·2	1·1	0·7	1·6	1·5	1·5	1·4	0·8
December 10	2·5	2·3	1·3	1·4	2·2	2·0	1·8	1·6	1·0
January 7	2·6	2·6	1·5	1·7	2·4	2·3	1·9	1·7	1·2
February 4	2·8	2·5	1·4	1·7	2·5	2·3	1·9	1·6	1·0

The preceding examples have not been selected to show the large errors in a ship's longitude which might result from the use of very bad instruments, but rather that in what are considered good and carefully regulated chronometers errors may, with adequate means for testing, be detected, and tables of corrections supplied to the mariner.

Examples 1 and 2, Table I., show how nearly it is possible to compensate for change of temperature between 55° and 85°. Some chronometers so compensated, when exposed to a temperature of 40°, change their rates very much, while in others the alteration of rate is comparatively small.

On ascertaining the chronometrical difference of longitude between the Liverpool Observatory and the Observatory at Cambridge, Massachusetts, the late Professor W. C. Bond at the commencement employed twelve marine chronometers which had been used previously on several occasions for obtaining differences of longitude. During the voyages between Liverpool and Boston, in the summer months, the sea and shore rates of these chronometers were sensibly the same; but during the winter months they differed considerably. On testing these instruments in 40° and 60°, the following results were obtained:—

Table V.—Showing the increase of gaining-rate of twelve chronometers caused by changing the temperature from 40° to 60°.

No.	Increase of mean daily rate, gaining.	No.	Increase of mean daily rate, gaining.
	s.		s.
1...........	7·6	7...........	3·0
2...........	5·6	8...........	2·7
3...........	4·8	9...........	1·5
4...........	3·7	10...........	1·3
5...........	3.4	11...........	0·8
6...........	3·0	12...........	−3·5

The chronometers alluded to in this Table were made by the late Mr.

Dent, and used by him for finding the longitudes of several observatories in this country.

On testing 100 chronometers in succession as they passed through the Observatory, the average alteration of daily rate caused by changing the temperature from 40° to 60° was $7^{s}\cdot 0$; and in ten per cent. of the hundred the average change was $30^{s}\cdot 6$.

The chronometer-room at the new Observatory now being erected at Bidston by the Mersey Docks and Harbour Board will be provided with the means of testing simultaneously between two and three hundred chronometers in the way shown by the examples in Table I. It is not necessary to test chronometers in this elaborate way on every occasion that they arrive in port, as the corrections for change of temperature remain the same for a long period. The *rate* may change, as in example 2, Table IV., while the thermal correction remains sensibly the same.

When the Greenwich mean time is communicated from an authorized establishment, as is now generally the case in our large sea-ports, the rates of chronometers in the temperature that prevails at the time can be easily ascertained. At present these rates are used on the assumption that the thermal adjustments are perfect. The corrections for change of temperature in Table II. show the improvement which might be effected by testing all chronometers when new, and supplying mariners with Tables of such corrections as may be found to exist. These corrections would require verifying periodically, as in cleaning and repairing timekeepers the thermal adjustment is sometimes altered.

December 21, 1865.

Sir HENRY HOLLAND, Bart., Vice-President, in the Chair.

The following communications were read:—

I. "On the Expansion of Water and Mercury." By A. MATTHIESSEN, F.R.S. Received December 7, 1865.

(Abstract.)

Before commencing a research into the expansion of the metals and their alloys, it was necessary to prove that the method I intended to employ, namely that of weighing the metal or alloy in water at different temperatures, would yield good and reliable results.

To check, therefore, the method, I was led to determine the coefficient of expansion of mercury, and, basing my calculations on Kopp's coefficients of expansion of water, I expected to obtain Regnault's coefficient of expansion of mercury. The coefficient deduced from experiments did not agree with Regnault's; and being unable to discover any source of error in the method of experimenting, I determined to reinvestigate the matter.

The memoir is divided into four parts.

I. On the determination of the coefficients of the linear expansion of certain glass rods.

These rods (1825 millims. long and of 20 millims. diameter) were kindly made for these experiments by Mr. F. Osler. The method used for the determination of their increment in length was that of measuring it with a micrometer-screw, with which a length could be measured with accuracy to 0·001 millim.

The rod was placed in a long trough, the one end of the rod resting against a fixed glass tube capped with zinc, the other against another glass tube the other end of which rested against the micrometer-screw. Water was allowed to flow through these glass tubes during the time of observation. The trough being filled with water at ordinary temperature and the position of the screw read off, the water was heated to boiling and another reading taken.

The mean of sixteen observations gave for the linear expansion of these rods

$$L_t = L_0 (1 + 0{\cdot}00000729t),$$

and therefore for the cubical expansion

$$V_t = V_0 (1 + 0{\cdot}00002187t).$$

II. On the method employed for the determination of the cubical expansion of water and mercury.

This part of the paper contains a full description of the apparatus employed, and the precautions taken.

The method consists of weighing the substances in water at different temperatures, and from the loss of weight in water deducing its volume. For this deduction, the expansion of water at different temperatures is required.

III. On the redeterminations of the coefficients of expansion of water.

To determine these, pieces of the glass rods (the linear expansion of which had to be determined), ground to the shape of a double wedge, were weighed in water of different temperatures. Three pieces of glass were used (making three Series), the weighings being made at temperatures between 0° and 100°, the whole number of observations being thirty-two.

From these it was found that the expansion of water between 4° and 100° may conveniently be expressed between 4° and 32° by the formula

$$V_t = 1 - 0{\cdot}0000025300\,(t-4) + 0{\cdot}0000083890\,(t-4)^2 - 0{\cdot}00000007173\,(t-4)^3;$$

and between 32° and 100° by

$$V_t = 0{\cdot}999695 + 0{\cdot}0000054724t^2 - 0{\cdot}000000011260t^3.$$

The values calculated from these formulæ for the volume occupied by water at different temperatures are given in Table I. from degree to degree, together with the differences for each degree.

TABLE I.

T°. C.	Volume of water at T°.	Difference per 1°.	T°. C.	Volume of water at T°.	Difference per 1°.	T°. C.	Volume of water at T°.	Difference per 1°.
4	1·000000	0·000006	37	1·006616	0·000355	69	1·022050	0·000598
5	1·000006	22	38	1·006979	363	70	1·022648	604
6	1·000028	38	39	1·007351	372	71	1·023252	609
7	1·000066	53	40	1·007730	379	72	1·023861	616
8	1·000119	69	41	1·008118	388	73	1·024477	622
9	1·000188	83	42	1·008514	396	74	1·025099	628
10	1·000271	98	43	1·008918	404	75	1·025727	634
11	1·000369	110	44	1·009331	413	76	1·026361	639
12	1·000479	125	45	1·009751	420	77	1·027000	646
13	1·000604	138	46	1·010179	428	78	1·027646	650
14	1·000742	150	47	1·010614	435	79	1·028296	657
15	1·000892	162	48	1·011059	445	80	1·028953	662
16	1·001054	173	49	1·011510	451	81	1·029615	668
17	1·001227	185	50	1·011969	459	82	1·030283	673
18	1·001412	196	51	1·012435	466	83	1·030956	678
19	1·001608	206	52	1·012909	474	84	1·031634	684
20	1·001814	215	53	1·013391	482	85	1·032318	689
21	1·002029	225	54	1·013879	488	86	1·033007	694
22	1·002254	234	55	1·014376	497	87	1·033701	699
23	1·002488	243	56	1·014879	503	88	1·034400	704
24	1·002731	251	57	1·015390	511	89	1·035104	709
25	1·002982	259	58	1·015907	517	90	1·035813	714
26	1·003241	266	59	1·016432	525	91	1·036527	718
27	1·003507	273	60	1·016964	532	92	1·037245	724
28	1·003780	279	61	1·017502	538	93	1·037969	728
29	1·004059	286	62	1·018047	545	94	1·038697	732
30	1·004345	290	63	1·018599	552	95	1·039429	737
31	1·004635	296	64	1·019158	559	96	1·040166	741
32	1·004931	318	65	1·019724	566	97	1·040907	746
33	1·005249	329	66	1·020296	572	98	1·041653	751
34	1·005578	338	67	1·020874	578	99	1·042404	0·000755
35	1·005916	0·000345	68	1·021459	585	100	1·043159	
36	1·006261				0·000591			

IV. On the redetermination of the coefficient of expansion of mercury.

The pure mercury was weighed in a bucket in the water at different temperatures. The glass bucket was made from the end of a test-tube (its length being about 20 millims. and width 15 millims.). The expansion of this sort of glass was found to be

$$V_t = V_0\,(1 + 0{\cdot}00002566t).$$

Five series were made with mercury; and its expansions, deduced from the water-expansions given in Table I, were

Series I. . . . . . . $V_t = V_0\,(1 + 0{\cdot}0001815t)$,
Series II. . . . . . . $V_t = V_0\,(1 + 0{\cdot}0001813t)$,
Series III. . . . . . . $V_t = V_0\,(1 + 0{\cdot}0001808t)$,
Series IV. . . . . . . $V_t = V_0\,(1 + 0{\cdot}0001808t)$,
Series V. . . . . . . $V_t = V_0\,(1 + 0{\cdot}0001816t)$,
Mean . . . . $V_t = V_0\,(1 + 0{\cdot}0001812t)$,

a value closely agreeing with Regnault's, namely

$$V = V_0\,(1 + 0{\cdot}0001815t).$$

Calculating from the five series the coefficients of expansion of mercury, using Kopp's water-expansion (taking the volume at $4° = 1$), we find as mean

$$V_t = V_0 (1 + 0{\cdot}000178t).$$

In the following Table I give the values obtained by different observers for the volumes occupied by water at different temperatures, the volume at 4° being taken equal to 1.

TABLE II.

T.	p*.	Despretz †.	Pierre ‡.	Hagen §.	Matthiessen.
4°	1·000000	1·000000	1·000000	1·000000	1·000000
10	1·000247	1·000268	1·000271	1·000269	1·000271
15	1·000818	1·000875	1·000850	1·000849	1·000892
20	1·001690	1·001790	1·001717	1·001721	1·001814
30	1·004187	1·004330	1·004195	1·004250	1·004345
40	1·007654	1·007730	1·007636	1·007711	1·007730
50	1·011890	1·012050	1·011939	1·011994	1·011969
60	1·016715	1·016980	1·017243	1·017001	1·016964
70	1·022371	1·022550	1·023064	1·022675	1·022648
80	1·028707	1·028850	1·029486	1·028932	1·028953
90	1·035524	1·035660	1·036421	1·035715	1·035813
100	1·043114	1·043150	1·043777	1·042969	1·043159

Kopp, Despretz, and Pierre used the same method for their determinations—that of determining the expansion of water in glass vessels (dilatometers). Hagen employed the weighing process, but at high temperatures employed no special precautions to prevent the steam condensing on his fine wire ; hence his values at 90° and 100° fall below mine.

It will be seen from the foregoing Table that Kopp's values are lower than the others ; and bearing in mind that the coefficient of expansion of mercury, when deduced by means of these, falls below that obtained by Regnault, but when deduced from Despretz's or my own agrees closely with Regnault's, we are led to conclude that Kopp's values must be somewhat incorrect.

* Pogg. Ann. xcii. 42. † Ann. de Chim. et de Phys. lxx. (1re sér.) 1.

‡ Ann. de Chim. et de Phys. xiii. (3me sér.) 325. Calculated by Frankenheim, Pogg. Ann. xcvi. 451.

§ Abhandlungen d. k. Acad. der Wissensch. zu Berlin, 1865.

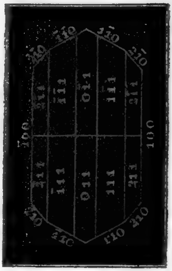
II. "On the forms of some Compounds of Thallium." By W. H. Miller, M.A., For. Sec. R.S., Professor of Mineralogy in the University of Cambridge. Received December 13, 1865.

Nitrate of Thallium.

Prismatic, 0 1 0, 0 1 1=38° 8′·1 ; 1 0 0, 1 1 0=62° 56′·3.

1 0 0, 0 1 1	90°	0′
1 0 0, 1 1 0	62	56·3
1 0 0, 2 1 0	44	23
1 0 0, 1 1 1	68	6·5
1 0 0, 2 1 1	51	13
1 1 0, 1 1 1	34	57·5
0 1 1, 0 $\bar{1}$ 1	103	44
0 1 1, 2 1 1	38	47
1 1 0, $\bar{1}$ 1 0	54	7·4
2 1 0, 2 1 1	28	46
2 1 0, $\bar{2}$ 1 0	91	14
0 1 1, 1 1 1	21	53·5
1 1 1, $\bar{1}$ 1 1	43	47
1 1 1, 1 $\bar{1}$ 1	93	44·8
1 1 1, $\bar{1}$ $\bar{1}$ 1	110	5
2 1 1, $\bar{2}$ 1 1	77	34
2 1 1, 2 $\bar{1}$ 1	75	38
2 1 1, $\bar{2}$ $\bar{1}$ 1	122	28

Fig. 1.



Observed combinations :—1 0 0, 1 1 1 ; 1 0 0, 1 1 1, 2 1 1 ; 1 0 0, 0 1 1, 1 1 1, 2 1 1; 1 0 0, 1 1 0, 2 1 0, 1 1 1, 2 1 1; 1 0 0, 0 1 1, 1 1 0, 2 1 0, 1 1 1, 2 1 1.

No cleavage observable.

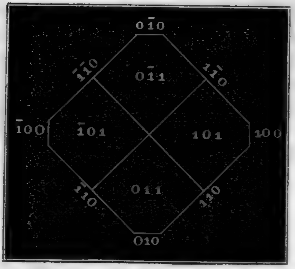
From the observed minimum deviation of the brightest part of the solar spectrum formed by refraction through the faces 1 0 0, $\bar{1}$ 1 0, it appears that the index of refraction of a ray in the plane 0 0 1, and polarized in that plane, is about 1·817. The refrangibility of the other ray is greater, its minimum deviation through the same faces being 93° nearly.

Sulphocyanide of Thallium.

Pyramidal, 0 0 1, 1 0 1=38° 20′·3.
Observed forms :—1 0 0, 1 1 0, 1 0 1.

	°	′
1 0 0, 0 1 0	90	0
1 0 0, 1 1 0	45	0
1 0 0, 0 1 1	90	0
1 0 0, 1 0 1	51	39·7
1 1 0, 1 0 1	63	59
1 0 1, $\bar{1}$ 0 1	76	40·6
1 0 1, 0 1 1	52	2

Fig. 2.



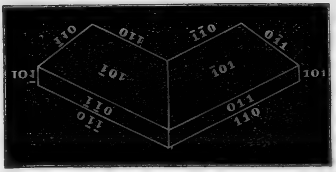
Observed combinations :—1 1 0, 1 0 1 ; 1 0 0, 1 1 0, 1 0 1.

The crystals are remarkable for the very unequal extension of the faces of the same simple form, and at first sight look as if they belonged to the oblique system. The breadth and thickness of one of the largest crystals were 1·1 and 0·055 millimètre respectively; and of two adjacent faces of the form 1 0 1, one was about eleven times the breadth of the other. The distribution of the large and small faces did not appear to be subject to any law; so that these crystals cannot be regarded as combinations of large and small hemihedral forms.

Twins. Twin face 1 0 1.

	°	′
1 0 1, $\underline{1}$ 0 $\underline{1}$	180	0
1 1 0, 0 $\underline{1}$ $\underline{1}$	52	4
$\bar{1}$ $\bar{1}$ 0, 0 1 1	−52	4
0 $\bar{1}$ 1, $\underline{1}$ 1 0	75	56
0 1 1, $\underline{1}$ $\underline{1}$ 0	75	56
$\bar{1}$ 0 1, $\underline{1}$ 0 1	26	38·8

Fig. 3.



No cleavage observable.

An attempt was made to determine the optical constants of the crystal by observing the minimum deviation of light refracted through a face of the form 1 1 0 and one of the opposite faces of the form 1 0 0 ; the latter were, however, so small that the observation could not be made with much accuracy. It appeared that for the ordinary ray polarized in a plane parallel to the line 0 0 1, the indices of refraction of red light, of the brightest part of the spectrum, and of violet light were about 2·115, 2·159, and 2·314 respectively, and that, for the extraordinary ray polarized in the plane 0 0 1, the indices of refraction of red light, the brightest part of the spectrum, and of violet light were about 1·890, 1·973, and 2·143 respectively.

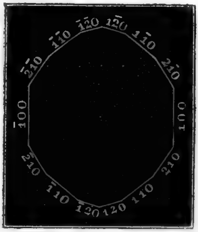
Carbonate of Thallium.

The faces which have been observed are all in one zone, and exhibit a symmetry which is compatible with either the prismatic or the oblique system. The crystals probably belong to the prismatic system. They are aggregated in such a manner as to render it very difficult to isolate a single crystal, or to determine the faces which belong to the different individuals of a group of crystals.

Observed forms :—1 0 0, 1 1 0, 2 1 0, 1 2 0.

1 0 0, 1 1 0	51° 28′
1 0 0, 2 1 0	32 7
1 0 0, 1 2 0	68 57
1 1 0, $\bar{1}$ 1 0	77 4

Fig. 4.



Twins. Twin face 1 1 0. One individual is generally united to each of two others, in this respect resembling the twins of cerussite, aragonite, glaserite, and chrysoberyl.

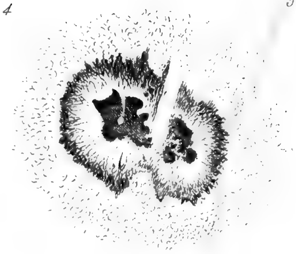
A cleavage has been observed probably parallel to the faces of the form 1 1 0; it may, however, be parallel to the faces of the form 1 0 0, the complexity of the twin crystals being such that it could not be ascertained whether the cleavages observed belonged to one crystal or to two different crystals.

I am indebted to Mr. Crookes, the discoverer of thallium, for the crystals of nitrate, sulphocyanide, and carbonate of thallium, above described.

J. Phillips ad nat. del.

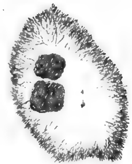
J. Basire lith.

4



1865
Novr. 11
9.45

5



1865
Novr. 13
9.45

6

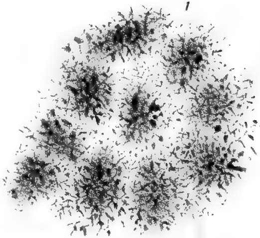
7

1865
Novr. 15
9.45

J. Phillips ad. nat. del.

J. Basire lith.

1



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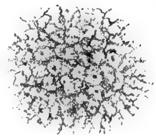

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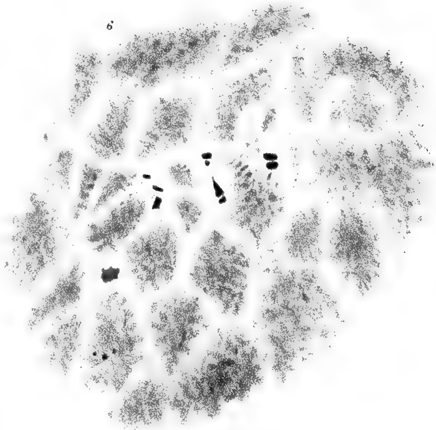

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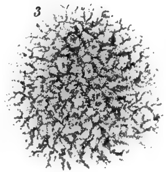

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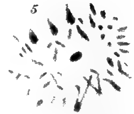

J. Phillips ad. nat. del.

Wind. N.W. Cloudy

J. Basire lith.

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END OF THE FOURTEENTH VOLUME.

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ERRATA.

Page 178, line 4 from the bottom, *for* George Child, M.D. *read* Gilbert W. Child, M.D.
" 228, " 12, *for* 618 *read* 619.
" 318, " 7 from bottom, *for* recognize *read* fail to recognize.
" 445, " 23, *for* eat *read* sweat.
" 446, " 2, " th *read* their.
" 449, " 2, " Yellowhammer *read* Yellow ammer.
" 455, " 25, " yellow-hammer *read* yellow ammer.
" 456, " 11, " Brains *read* Brain.
" — " 25, " exterior *read* exclusive.
" — " 32, " formed *read* freed.
" — " 33, " or *read* a.
" 475, " 5 from bottom, and page 513, line 3 from bottom, *for* Paul E. Count de Strzlecki *read* Paul E. Count de Strzelecki.
" 540, " 24, *for* receptionf *read* reception of.

NOTICE TO THE BINDER.

In this Volume the following pages are to be cancelled:—153 & 154, 203 & 204, 299, 357 & 475.

OBITUARY NOTICES OF FELLOWS DECEASED

BETWEEN 30TH NOV. 1863 AND 30TH NOV. 1864.

Capt. WILLIAM ALLEN entered the Navy in 1805. At the passage of the Dardanelles, by Sir John Duckworth, he served on board 'The Standard;' and afterwards took part in the expedition against Java. He was engaged in the Niger exploration under Capt. Trotter in 1841, and in 1848 published an account of the Voyage, in two volumes. In 1855 he brought out another work on the "Dead Sea, and the Overland Communication with the East," in which he recommended the cutting of a canal from the Mediterranean to the Dead Sea. He was an active member of the Royal Geographical Society, and was elected into the Royal Society in 1844. He died in January, aged seventy-one.

In the Rev. Dr. WILLIAM CURETON, Canon of Westminster, ancient literature has lost one of the ablest of Syriac scholars. His 'Corpus Ignatianum,' an edition of an ancient Syriac version of the Epistles of St. Ignatius, with commentaries, published in 1845, established his reputation as an Orientalist, and became the occasion of a spirited controversy which was carried on for some years among students of ancient texts. This was followed by an edition of a palimpsest of portions of Homer, discovered in a convent in the Levant, and in 1855 by 'Spicilegium Syriacum,' in both of which Dr. Cureton exhibited profound and accurate scholarship. He was continuing his researches into old Syriac versions of St. Matthew's Gospel at the time of his decease; and, considering how valuable were the services he rendered to that department of literature, the accident by which those services were interrupted is the more to be deplored.

Dr. Cureton was born in 1808. About two years before his death, which took place at Westbury, Shropshire, on June 17, 1864, he sustained so severe a shock from an accident to a railway-train in which he was travelling, that his health remained permanently impaired. He was educated at Christ Church, Oxford, and was ordained a priest in 1834; in 1847 he was appointed Chaplain in Ordinary to the Queen, and in 1849 was preferred to a canonry of Westminster, and therewith to the rectorship of St. Margaret's. Besides these ecclesiastical employments, he held for a short time the place of Sublibrarian to the Bodleian Library; in 1837 he became Assistant-keeper of the MSS. in the British Museum, and was afterwards appointed one of the Trustees of the Museum on the part of the Crown. He was elected a Fellow of the Royal Society in 1838.

JOSEPH HENRY GREEN was born in London, on the 1st of November, 1791, and died at Hadley, Middlesex, on the 13th of December, 1863.

Mr. Green's father was a merchant of high standing in the City of London, and his mother was a sister of Mr. Cline, the eminent surgeon. His school education was begun in this country, but completed in Germany, where, accompanied by his mother, he spent three years, chiefly in Hanover.

At the age of eighteen he was apprenticed to his uncle, Mr. Cline, and entered on the study of medicine at St. Thomas's Hospital, of which Mr. Cline was surgeon. In 1813 he married Miss Anne Eliza Hammond. This lady, who survives him, was the daughter of Mr. Hammond, surgeon at Southgate, and sister of an early friend and fellow student.

In 1815 Mr. Green became a member of the College of Surgeons, and was soon afterwards appointed Demonstrator of Anatomy at St. Thomas's Hospital. While in this office he published a 'Dissector's Manual,' which bore advantageous comparison with the books of the same kind then in use. In the meantime Mr. Cline had retired from St. Thomas's, and was succeeded by his son Mr. Henry Cline, on whose early death, in 1820, Mr. Green was appointed Surgeon to that Hospital, and Lecturer on Surgery in the Medical School, in conjunction with Sir Astley Cooper, who withdrew from the joint office in 1825.

The advantageous position in which Mr. Green was now placed, and his own merit, speedily gained for him the confidence of his profession and the public. In 1824 he was appointed Professor of Anatomy to the Royal College of Surgeons; in 1825 he was elected a Fellow of the Royal Society (in later years he served on the Council). Also in 1825 he received the appointment of Professor of Anatomy to the Royal Academy, and in the latter part of that year delivered the first of a long succession of annual courses on Anatomy in its relation to the Fine Arts. Ere now, too, he had acquired a considerable and increasing share in the private practice of his profession.

Respecting the Lectures at the College of Surgeons, which formed one comprehensive course distributed over four years, Professor Owen, who heard them delivered, thus writes to Mr. Simon* :—"For the first time in England the comparative anatomy of the whole animal kingdom was described, and illustrated by such a series of enlarged and coloured diagrams as had never before been seen. The vast array of facts was linked by reference to the underlying Unity, as it had been advocated by Oken and Carus. The Comparative Anatomy of the latter was the text-book of the Course. . . . . . Green illustrated, in his grand course, Carus rather than Hunter; the dawning philosophy of Anatomy in Germany, rather than the teleology which Abernethy and Carlisle had previously given as Hunterian, not knowing their master."

Of Mr. Green's lectures at the Royal Academy (where he retained his professorship till 1852), Mr. Simon, who attended several of the courses, thus expresses himself:—"His teaching at the Royal Academy, like all his teaching, was characterized by a very deep-going and comprehensive treatment of his subject. He recognized, of course, that the details of anatomy

* The facts, and in some places the language, of this notice have been taken from a biographical memoir prefixed to Mr. Green's posthumous work (to be afterwards referred to) by its editor, John Simon, Esq., F.R.S., Mr. Green's friend and pupil. The passages in inverted commas are taken from that source.

(even of mere artistic surface-anatomy) could not be adequately spoken of, much less conveyed, in the six formal lectures which he had annually to deliver. . . . . . . Not indeed that he omitted to survey, or surveyed otherwise than admirably, the composition and mechanism of the human body; and perhaps no mere anatomist ever taught more effectively than he what are the bodily materials and arrangement which represent the aptitude for strength, equipoise, and grace, or what respective shares are contributed by bone, muscle, and tegument to the various visible phenomena of form and gesture, attitude and action. But to this he did not confine himself. Specially in the one or two introductory or closing lectures of each course, but at times also by digression in other lectures, he set before his hearers that which to them, as artists, was matter of at least equal concern—the science of interpreting human expression and appreciating human beauty. His discourses on these subjects were very deeply considered. Necessarily they were of wide philosophical range. And they were enriched with numberless illustrative references to the history of Art, and to the master-works of ancient and modern sculpture and painting."

On the establishment of King's College in 1830, Mr. Green was nominated Professor of Surgery, and continued to hold the Professorship till 1836, when he resigned it (on retiring to live in the country), and was elected a Member of the Governing Council of the institution. Of his surgical lectures it is stated on the best authority that the technical instruction imparted, perfect as it was, was by no means their sole excellence; they had also a moral aim, and were admirably fitted to exert a favourable influence on the habits of thought and future professional character of his young hearers.

In 1835 Mr. Green was elected on the Council of the College of Surgeons, and in 1846 appointed to the Court of Examiners. In 1840 and 1847 he delivered the Hunterian Oration; in 1849–50 and again in 1858–59 he was President; in 1853 he exchanged his post of Surgeon to St. Thomas's for the honorary appointment (then first made) of Consulting Surgeon to that institution; and on the creation, by the Medical Act of 1858, of the General Council of Medical Education and Registration, he was chosen by the College of Surgeons to be its representative in the new body. Two years later, when the post of President of the Medical Council became vacant by the retirement of Sir Benjamin Brodie, the Council unanimously elected Mr. Green to the office; and he continued in it, with the warmest regard and confidence of its members, for the remaining three years of his life.

Mr. Green thus attained to the foremost rank in his profession, and came to occupy with universal assent its highest public offices; but the contemplation of his professional and public career would convey a wholly inadequate notion of his intrinsic mental tendencies and pursuits, and the scope of his intellectual activity. From his early years he had a bent towards the study of abstract philosophy in its largest and highest sense; and to gratify

this inclination he, in the summer of 1817, found time to spend a few months in Berlin to go through a private course of reading on philosophy with Professor Solger, on whom, as well as on Ludwig Tieck whom he had met in London, his amiable disposition and "noble eagerness for knowledge" made a most favourable impression. Probably about this time also he became acquainted with Coleridge, and contracted an admiration of his philosophy; soon afterwards, at any rate, a close intimacy grew up between them, which continued during the rest of Coleridge's life. "Invariably he spent with Coleridge—they two alone at their work—many hours of every week, in talk of pupil and master. And so year after year, he sat at the feet of his Gamaliel, getting more and more insight of the teacher's beliefs and aspirations, till, in 1834, two events occurred which determined the remaining course of his life. On the one hand, his father died, and he became possessed of amply sufficient means for his profession to be no longer needful to his maintenance. On the other hand, Coleridge himself died. And the language of Coleridge's last will and testament, together no doubt with verbal communications which had passed, imposed on Mr. Green what he accepted as an obligation to devote, so far as necessary, the whole remaining strength and earnestness of his life to the one task of systematizing, developing, and establishing the doctrines of the Coleridgian philosophy."

Influenced by these circumstances he withdrew from private practice and resigned his professorship at King's College. Then, too, he gave up his London house and retired to reside at Hadley; and although he did not relinquish his interest in the practical aspects of his profession or his care for the amendment of its institutions, continuing still to take an active share in the government of the College of Surgeons, and finally presiding in the Medical Council, yet all such occupations and objects then became secondary in his mind to the one object of his philosophical studies and the fulfilment of the task he had undertaken.

With this purpose Mr. Green entered upon the widest possible range of study; for he deemed it necessary to test the applicability of the Coleridgian system to all branches of methodized human knowledge. Accordingly, in the twenty-seven years of life that remained to him, "Theology, Ethics, Politics and Political History, Ethnology, Language, Æsthetics, Psychology, Physics and the Allied Sciences, Biology, Logic, Mathematics, Pathology—all were thoughtfully studied by him in at least their basial principles and metaphysics, and most were elaborately written of as though for the divisions of some vast encyclopædic work."

Mr. Green took advantage of the public discourses which on more than one occasion he was called on to deliver, to make known his opinions on the relation of the Coleridgian philosophy to the study of science and the learned professions. Of these there have appeared in print his Address on the opening of the Medical Session at King's College in 1832, the Hunterian Oration for 1840, entitled "Vital Dynamics," and that for 1847,

with the title "Mental Dynamics." But as years advanced, certain threatening bodily ailments warned him that it was time to utilize in a systematic and communicable form, at least a part of the fruits of his vast preparatory labour; and he accordingly determined to complete a work which should give in system the doctrines, especially the theological and ethical doctrines, which he deemed most distinctively Coleridgian; and to this he devoted what in effect proved to be the whole available remainder of his life. The result is a work in two volumes published under the editorship of Mr. Simon. The first volume is devoted to the general principles of philosophy, while the second is entirely theological, and especially aims at vindicating *à priori* (on principles for which the first volume has contended), the essential doctrines of Christianity.

The mental qualities and character of Mr. Green will be found ably delineated in Mr. Simon's memoir; suffice it here to say that his life, both private and public, was a life of benevolence, probity, truth, and honour.

Mr. Hudson Gurney, who died at the advanced age of ninety-five, was one of the well-known Norfolk family of that name, members of the Society of Friends, and through his wife was connected with the Barclays of Ury. He was for many years a leading Member of the House of Commons, distinguished by the favour he showed to men of letters, and the literary and art collections which he formed. In 1811 he published a poem, 'Cupid and Psyche,' based on the Golden Ass of Apuleius. He was elected a Fellow of the Royal Society in 1818.

Leonard Horner, the third and youngest son of Mr. John Horner, linen-merchant in Edinburgh, was born on the 17th of January, 1785. It was but natural that with an early enthusiasm for science he should have become a geologist; for in Edinburgh at that time Hutton, Hall, Playfair, and a band of zealous followers, by observation in the field and by experiment in the workshop, were gathering materials for a new philosophy of geology, and were waging a keen warfare with the partizans of Werner. The year of Mr. Horner's birth was that in which Hutton's famous excursion to the granite of Glen Tilt was made. He was three years old when that philosopher unfolded his new theory to the Royal Society of Edinburgh, and he had grown up to be a High School boy of ten years of age when the immortal 'Theory of the Earth' was published. At that time, indeed, according to his own confession, he was a thoughtless youth with no special liking for study, and a vague passion for the sea. But these scientific discussions had not come to a close when he grew up to be able to understand and take an interest in them, and their influence is to be traced throughout his life. He entered the University of Edinburgh in 1799, and attended the lectures of Playfair on Mathematics. In 1802 he was studying moral philosophy under Dugald Stewart, and chemistry with Hope; and it was when fairly launched into these studies that his mind took that

bent towards natural science by which it was marked during the rest of his life. "From that time," he writes, "began a new state of mind. I took an interest in the subject, bought apparatus, made experiments, and destroyed many of my mother's towels. I took a particular interest in mineralogy, began to make a collection of specimens, cultivated acquaintence with some fellow students who had the same turn, and read Playfair's 'Illustrations of the Huttonian Theory,' of which I became a worshipper, having heard it well expounded by Dr. Hope." He was too young to have personal intercourse with Hutton, though he tells how he used to hear much in his own family of the "ingenuity, acuteness, and even lighthearted playfulness" of that philosopher. But he became attached to the Professor of Mathematics, to whom sixty years afterwards he referred from the chair of the Geological Society as his "venerable friend the able and eloquent Playfair."

At the age of nineteen Mr. Horner left Edinburgh to become partner in a branch of his father's business, which it was proposed to carry on in London. His elder brother Francis was already rising to eminence in the House of Commons; so that Mr. Horner soon found himself in the midst of a large circle of friends, among whom were not a few of note in science and literature. Two years afterwards he married Miss Lloyd, daughter of a landed proprietor in Yorkshire, and took a house in London. His love for geology, however, was not quenched by the claims of business, for we find him, the year after his marriage, joining the newly-founded Geological Society. Nor did he become an inactive member. In 1810, the second year after his election, he was chosen one of the Secretaries of the Society, and from that time down almost to the very day of his death, he continued one of its most zealous and unwearying members.

In 1815 he found himself under the necessity of returning to Edinburgh to take a personal superintendence of his business there. Two years afterwards his brother Francis, with whom he had journeyed to Italy in a vain search for health, died full of promise. When Mr. Horner had recovered from the blow of this sad loss, his active mind sought new scope for itself in the organization of political meetings, wherein the young Whiggism was developed, for which Edinburgh afterwards came to be so noted. In this, as in many other features of his life, Mr. Horner showed the practical and methodical character of his mind, as well as his social disposition; for these meetings were not arranged without exciting much keen opposition and political feeling. His residence in Edinburgh was marked by the success of another project—one of the most widely useful of all his schemes for the benefit of his fellow-men. In March 1821, happening to observe some watchmakers at work, he was led to inquire whether they ever received any mathematical education. On being told that they did not, and that, though anxious to obtain such instruction, they could not afford to pay for it, the idea occurred to him to found a school for the training of mechanics in those branches of science which would aid them in their daily work. Hence

arose the Edinburgh School of Arts. Mr. Horner laboured hard for the success of this scheme, and he lived to see it completely successful. He acted as Secretary of the School for the first six years; and during all the rest of his life, even though no longer resident in Edinburgh, he continued to take an active interest in the institution and in its prominent students. He several times gave donations of books to the library, and in 1858 invested a sum of money for an annual prize of three guineas. The usefulness of this school has been great. About seven hundred young men are entered annually as students in mathematics, chemistry, or natural philosophy, and receive at small cost instruction which would otherwise lie beyond their reach. Several of the foremost engineers of the present day have been students there. It was in remembrance of this and similar kinds of philanthropic activity, that Lord Cockburn styled Mr. Horner "one of the most useful citizens Edinburgh ever possessed."

Mr. Horner left Edinburgh in the year 1827 to assume the office of Warden in the University of London, a post at which he laboured for four years, until his failing health led him to seek a retreat with his family on the banks of the Rhine. At Bonn he had leisure to renew his old love for mineralogical and physical geology; and in making himself acquainted with the geological structure of the district, he at the same time formed a lifelong friendship with some of the most eminent men of science and learning there. On his return to England in 1833 he was appointed one of a Commission to inquire into the employment of children in the factories of Great Britain. The Report of this Commission gave rise to the Factory Act, under which Mr. Horner was made one of the Inspectors of Factories, an office which, through good and ill report, he laboriously and conscientiously filled for nearly thirty years. His zeal for the interests of the women and children in the factories often placed him in conditions of great delicacy, yet, notwithstanding opposition and disparagement, he continued his exertions, and earned the gratitude of the workers, while he was at the same time rewarded by finding an ever-increasing number of millowners who acknowledged the benefits of the Act which it was his duty to enforce.

During these busy years, however, he never lost or relinquished his interest in the progress of science, and more especially of Geology. No face was more constantly seen at the Meetings of the Royal and Geological Societies than that of Mr. Horner. He had become a Fellow of the Royal Society in 1813, and in various years served on the Council. In 1845 he took an active part in the reform of the Society, whereby the mode of Election of new Members was modified. In the year 1857 he was nominated Vice-President. In the Geological Society he took a still more prominent part. Besides reading papers at its Meetings, he became in 1846 its President, an office which he again filled in 1860. He was unremitting in his attention to all that might in any way further the interests or usefulness of the Society. He worked with his own hands in the Museum, arranging and cataloguing its stores of specimens; and he

carried on this task at intervals up to within a short period of his death, labouring often to the verge of his physical strength. To his suggestion is due the publication of the Quarterly Journal of Papers read at the Society's Meetings, one of the most important undertakings of this Society.

When Mr. Horner at last resigned the office of Inspector of Factories, although now seventy-five years of age, he still remained so full of youthful energy, that he looked forward hopefully to spend yet a few years in more undivided attention to his favourite science. Unable longer for the toils of out-of-door geology, he resumed with fresh zeal the arrangement of the Geological Society's Museum, anxious that its stores of rock-specimens should be classified in such a form as in the end to afford a comparative series of the different rocks throughout the globe. The failing health of his wife interrupted this task, and induced him to spend the winter of 1861–62 at Florence. There, as at Bonn, he found a ready welcome into the cultivated and learned society of that city. While there, he occupied himself with translating from the Italian Villari's 'Life of Savonarola,' and published it in England a few months afterwards. Mrs. Horner's health, however, which had continued a source of anxiety to him, at last gave way, and she died as the family was on the point of returning to England. When Mr Horner came back to London, his friends saw with concern that this great sorrow had told only too plainly upon his health. His strength began to fail, but his energy seemed as fresh as ever. He returned to his labours among the collections of the Geological Society, and day after day he was found poring over dusty specimens, describing and cataloguing them with the same perseverance and even enthusiasm which he had shown from the beginning. A few months after his return from Italy, viz. during the summer of 1862, he paid his last visit to his native city. Never was his welcome warmer. He came at the time when the schools were passing through their public examination previous to dismissal for the autumn holydays—the High School where he himself had been educated, and the Academy which, with Lord Cockburn, he had founded. He attended the examinations, addressed the boys, presented some of the prizes, and showed at the end of his long life the same deep interest in education and in the pursuits of youth. His old Edinburgh friends, too—now a yearly decreasing number—vied with each other in their attention to the venerable philanthropist.

Returning from Scotland to London, he fixed upon the 15th of March, 1864, as the day when he should leave England to revisit the grave of his wife at Florence. But before that day came round a cold seized him, followed by extreme weakness, and he died calmly on the 5th of March.

Physical geology was the branch of science to which Mr. Horner more specially devoted himself. The influence of his early acquaintance with Playfair and the Huttonian geologists at Edinburgh is visible throughout his scientific course. He began the study imbued with the prevailing

ideas regarding the importance of mineralogical geology; and his first papers—that on the Malvern Hills, and that on Somersetshire—may be taken as characteristic specimens of the mineralogical system of treatment by which the geology of the early part of this century was marked. But though from the state of the science at that time (1811–1815) it was not to be expected that he should succeed in unravelling the complicated geological relations of the different rocks, it is yet interesting to mark how he carried with him the spirit of careful observation in which Playfair had trained him, and how readily he saw among the hills of England proofs of the truth of the Huttonian system. During his active life he had few opportunities of doing much in field-geology. When he found a little leisure in his retreat at Bonn, he at once reverted to his favourite science, and the results of his sojourn were given to the Geological Society in a paper on the Geology of the Environs of that town. During the same interval of rest he was led, in the true spirit of the Huttonian school, to institute a series of experiments on the quantity of solid matter suspended in the water of the Rhine, with the view of arriving at some "measure of the amount of abraded stone transported to the sea, there to constitute the materials of new strata now in progress of formation." These researches have become classic in the history of geology. Fifteen years later a similar kind of inquiry greatly interested him when Lepsius called attention to certain sculptured marks in the valley of the Nile; and in 1851 he obtained from the Royal Society a grant of money for the purpose of excavations to be made in the Nile alluvium. To link together the earliest human with the latest geological history seemed to him an object worthy of earnest prosecution. After four years of exploration, carried on according to a plan drawn up and sent out by him to Egypt, Mr. Horner published the results of his researches in the 'Philosophical Transactions' for 1855. His presidential addresses to the Geological Society were devoted to a survey of the progress of geology. They are remarkable for the sympathy which they show for views far in advance of those in which he had himself been trained.

But it is not by the number or character of his writings that Mr. Horner's influence among the scientific men of his day is to be estimated. His age and experience, his association with the early days of British geology, his political connexions, his sound judgment and careful business habits, joined to his excellent social qualities, gave him a position which none can now fill. And he retained his influence in no small measure from the singular fervour and youthfulness of his mind. Instead of clinging to old methods and beliefs as one of his years and early predilections might have been expected to do, he was found ever ready to receive and sympathize with new developments of truth, and to uphold the cause of progress in all departments of science. Even at the last, when he read his final address to the Geological Society, he pleaded boldly for the high antiquity of the human race in opposition to popular prejudice on this subject, and

claimed for the speculations of Mr. Darwin the thoughtful consideration of all lovers of truth. Mr. Horner's death severed a link closely and visibly connecting the geologists of today with the early masters of the science in this country, and closed a long and honourable life, full of all kindliness, and ever devoted to the welfare of his fellow men.

Luke Howard was born in London in 1772, a date which carries us back to the early years of the reign of George III., and opens a long vista of history in which great political changes are rivalled by the grandest discoveries of modern science.

Luke Howard's parents, members of the Society of Friends, sent their son to a country school in North Oxfordshire, where, as he was accustomed to say in after life, "he learnt too much of Latin grammar and too little of anything else." But having even then an observing eye, he began to notice the appearances of the sky and forms of clouds; and his inclination towards meteorology appears to have been fixed by his impressions of the remarkable atmospheric and meteoric phenomena which, as those acquainted with the history of meteorology will remember, characterized the year 1783.

From school young Howard went as apprentice to a chemist at Stockport, which was then a quiet country town. In this situation he devoted his spare hours to the course of self-improvement which he had already begun, and acquired that knowledge of French, botany, and the principles of chemistry, which were so useful to him in after years. The quickening effect produced on his mind by the works of Lavoisier he described as "like sunrise after morning moonlight," an effect which has been felt by many a student.

In 1798 he entered into partnership with William Allen, whose reputation as a manufacturing chemist has long been recognized. This connexion, however, was brought to an amicable close a few years later, and Howard, taking as his portion the laboratory at Plaistow, applied himself to the business therewith connected, and to his favourite scientific pursuits. Making use of his observations of natural phenomena, he wrote a paper "On the Modifications of Clouds," and read it at a meeting of the Askesian Society, of which he and his friend Allen were members. This paper, as he himself tells us, "the result of his early boyish musings, enriched by the observations of many a walk or ride, morning and evening, to or from his day's work at the laboratory," was published in 1803, and made known the author's name and ability to a wider circle. The Askesian was not a publishing Society; otherwise Luke Howard might have been better known than he is as a pioneer in departments of science besides meteorology. "I know," writes one of his friends, "that one or more of his papers related to atmospheric electricity, and another was an anticipation of the cell-theory, as regards the structure and functions of plants, founded on microscopic investigations."

Many, if not all, the articles on meteorology in 'Rees's Cyclopædia,' were written by Luke Howard. He contributed a series of papers to the 'Athenæum,' embodying the results of his meteorological observations from the year 1806; and these he published in two volumes (1818–20), under the title "Climate of London, deduced from Meteorological Observations made in the Neighbourhood." This, republished in 1833, in three volumes, has become one of our standard works on meteorology.

Luke Howard was elected a Fellow of the Royal Society in 1821. From that time his reputation as a meteorologist increased, and eminent persons in many parts of the world opened a correspondence with him, which, in some instances, became the initiation of a lasting friendship. Although the increasing perfection of philosophical apparatus has superseded some of his methods of observation, there can be no doubt that his labours imparted more of a scientific character to meteorology than it had ever received before. His classification of the clouds is the one still recognized at all observatories, and remains an evidence of the quick eye he had for form and colour, and of the daily labour which was to him a labour of love. One who knew him well in the latter part of his life, says, "Those who lived with him will not soon forget his interest in the appearance of the sky. Whether at morning, noon, or night, he would go out to look around on the heavens, and notice the changes going on. His intelligent remarks and pictorial descriptions gave a character to the scene never before realized by some. A beautiful sunset was a real and intense delight to him; he would stand at the window, change his position, go out of doors, and watch it to the last lingering ray; and long after he ceased, from failing memory, to name the 'cirrus,' or 'cumulus,' he would derive a mental feast from the gaze, and seem to recognize old friends in their outlines."

Sharing in the active beneficence so characteristic of the Society of Friends, Luke Howard readily aided endeavours for the religious and moral as well as the material welfare of the community. Not least important among these was the seeking to mitigate by pecuniary means the sufferings of the Germans during the campaigns immediately preceding the first abdication of Napoleon. In Ackworth School—a well-known establishment of the Friends—he took a lively interest; and to participate the more directly therein, as well as to offer hospitality to the annual visitors to the school, he bought the Ackworth Villa estate in 1823, making it his summer residence, and Tottenham his winter residence, during the greater part of his life.

In 1796 Luke Howard married Mariabella Eliot, a member of the same Society with which he was himself connected. Of their family of seven children two sons only survived their parents. About his eightieth year he was much enfeebled by alarming attacks of illness; and the death of his wife following, after a union of fifty-six years, added sorrow to his

weakness. Henceforward his life was a subdued waiting for the end. He died at Tottenham on the 21st March, 1864.

A portrait of Luke Howard, bequeathed to one of his friends, is eventually to be added to the Royal Society's collection. Besides the works above mentioned, he published—Essay on the Modifications of Clouds, 1832; Seven Lectures on Meteorology, 1837; a Cycle of Eighteen Years in the Seasons of Britain, &c., 1842; Barometrographia—Twenty Years' Variation of the Barometer in the Climate of Britain, 1847; Papers on Meteorology, 1850–54; and The Yorkshireman, a religious and literary Periodical, in 5 vols., 1833–37.

William Chadwell Mylne was born in London, on the 6th of April, 1781, and died on the 25th of December, 1863. His father, Robert Mylne, F.R.S., a native of Edinburgh, and the representative of a long line of Scotch architects, commenced his career in London in 1759 by building Blackfriars Bridge, and held the appointment of Engineer to the New River Water Works, to which his son, the subject of this notice, succeeded in 1810.

Mr. Mylne may be said to have been from his cradle bred an engineer. When a boy only sixteen years of age he was engaged with the younger Mr. Golborne in the Fen country in staking out the lands for his father's great scheme of the Eau Brink Cut, an undertaking which, through opposing interests, was defeated at that time, but was eventually carried out by Mr. Rennie in 1817. Subsequently he was occupied on his father's well-known project, the Gloucester and Berkeley Ship Canal, seventy feet in width; and he was generally engaged in assisting his father in the largest professional practice of that day.

Succeeding at thirty years of age to the sole conduct of the New River Works, Mr. Mylne had before him an arduous and responsible office. The supply of water to London had hitherto been solely derived from the New River and London Bridge Works; but the rapid extension of the metropolis led to the establishment of new companies, which gave rise to serious contests, and for some years involved them in a ruinous competition. Mr. Mylne's ability and energy were soon tried in carrying out extensive changes in the New River Works. The old wooden main pipes, which up to 1810 were the principal conduits for the passage of water, were found insufficient to stand the requisite pressure, and it was deemed expedient to substitute pipes of cast iron. This improvement was effected at a cost of nearly half a million sterling; and the whole was satisfactorily accomplished under Mr. Mylne's judicious management.

Notwithstanding the constant and unremitting engagements of the New River business, Mr. Mylne was occupied in considerable engineering practice, particularly in the Fen country, carrying out Sandys Cut, with several other important drainage works. Combining also the hereditary profession of an Architect, he was engaged in bridge-building, and in the alte-

rations and extensions of many private mansions. Among his works, the single-arched iron bridge over the River Cam, at St. John's College, Cambridge, has been much admired; and the church of St. Mark's, Clerkenwell, met with considerable approval at the period of its erection, forty years since.

Mr. Mylne in later years was much occupied in Government references, and acted as surveyor for fifty years to the Stationers' Company, having succeeded his father in that office. He was also extensively engaged before Parliamentary Committees on Water, Dock, and Drainage Works, and was consulted in continental works of similar character.

From the date of his entering on the direction of the New River Works to his retirement, two years before his death—a period of fifty years—he had the satisfaction to witness a very great advance in the income of the Company, and a great extension of their works, consequent on the increased demand caused by the further growth of the metropolis and awakened attention to its salubrity. In 1852 new works were undertaken to the extent of three quarters of a million sterling, and executed by him, with the assistance of his son, R. W. Mylne, F.R.S.

Mr. Mylne was a man of a peculiarly kind and conciliatory disposition, a peace-maker in all professional strife, of strict integrity and high honourable feeling. He was for many years the guiding hand, as Treasurer, to the Society of Engineers styled "Smeatonians," in which, as in all other Associations, he won the respect, esteem, and almost affection of those with whom he was connected. His retiring disposition caused him seldom to take part in scientific discussions; but he took a keen interest in all questions of progress, and during his long career judiciously availed himself of the opportunities offered him of adopting the new inventions of the age.

Mr. Mylne was elected a Fellow of the Royal Society on the 16th of March, 1826.

Major-General Joseph Ellison Portlock, son of Captain Nathaniel Portlock, a distinguished officer of the Royal Navy, was born at Gosport in September 1794. He received his early education at a school in his native town and at Tiverton, from which he went to the Royal Military Academy at Woolwich. In 1813 he took his first commission in the Royal Engineers, and was sent in the following year to Canada, where he remained actively employed in military service or exploring expeditions until 1822. He was present at the siege of Fort Erie; and, on the retirement of the troops, he constructed the lines and bridge-head at Chippewa, at which Sir Gordon Drummond made his successful stand, and saved Upper Canada.

In 1824, on the extension of the Ordnance Survey to Ireland, Lieut. Portlock was one of the officers first selected by Colonel Colby to take part in the work; and his earliest duty in connexion therewith, conjointly with Lieuts. Drummond and Larcom, lay in working out the preliminaries of

what has since grown into first-rate importance as the Topographical Department. The task at that time was beset by difficulties, which the progress of physical and mechanical science has since removed: the preparation of the base-apparatus, the construction of astronomical and other surveying-instruments, the contriving of signals by lamp and heliostat, and the training of sappers for their special duties had to be undertaken under the disadvantage of newness. But at that time the Duke of Wellington was Master-General of the Ordnance; and supported by him, Colonel Colby carried out his plans in full efficiency.

In 1825 the first detachments were removed to Ireland, and the first trigonometrical station was taken up on Divis Mountain, near Belfast. There the first signals and observations with lamp and heliostat were attempted, and, to the satisfaction of the originators, proved completely successful. This was Lieut. Portlock's start on the trigonometrical branch of the survey, of which he shortly became the senior, and eventually sole officer.

In addition to scientific skill and accuracy, great personal endurance was required in carrying on the observations. In 1826 the camp on Slieve Donard, 2800 feet above the sea, was more than once blown down by the violence of the wind. Colonel Colby was seriously injured by a fall while climbing from the observatory to his tent; and communication with the country below involved both difficulty and danger. Yet "Portlock," we are told, "held out to the last. For some weeks he was the only officer remaining; but he struggled on, and brought the operations to a successful close."

In the following year, while Colonel Colby was measuring the base on the shore of Lough Foyle, Lieut. Portlock, with Lieut. Larcom, carried out the observations at seven hill-stations, regardless of season and weather. In 1828, and for some years afterwards, he performed the work single-handed, observing with the great theodolite from mountain after mountain till the principal network of triangulation was complete, and the Irish system was, by means of the lamp and heliostat signals, united to that of Britain. In addition, care had to be taken for the direction of the secondary triangulation for the details of the survey, and for the rectification of errors and the discrepancies that were sure to occur at the junction of the separate districts. For this the whole had to be combined under one general system; and this additional labour Lieut. Portlock undertook while still on the mountains. He carried it on afterwards at his office in Dublin; and so well did he direct these secondary operations, that, after the parties became used to the work, the surveying went on at the rate of three million acres a year.

The horizontal survey involved the necessity of an elaborate vertical survey and calculations for altitude. The altitudes were deduced at first from the sea, by actual levelling from it to bases of altitude, and from them transferred, by angles of elevation and depression, to the summit of every

hill and station, at distances averaging a mile asunder; and on this the minor levelling of the detail survey depended. This also was ultimately generalized into a system by Lieut. Portlock, and by him furnished regularly and rapidly. In fulfilling this purpose, he personally carried a line of levelling across the island from the coast of Down to the coast of Donegal, and caused several lines to be observed in other places. In this way a more general and homogeneous system of altitudes was obtained than had ever before been attempted. It supplied the data for the paper on Tides by the Astronomer Royal, published in the 'Philosophical Transactions.'

In all this we see a character conspicuous alike for ability and energetic perseverance; but among its other elements, there was one which may be properly noticed here—the praiseworthy example he set to the men under his command. They felt that with him they were in the hands of something superior to themselves in intellect and acquirements, and they improved in a marked degree in the duties of the survey, in intelligence, and the habit of obedience. "They needed only encouragement, no coercion, and they rapidly acquired knowledge; to all of which I can testify," writes one of Portlock's brother officers; "and I am sure it is the experience of the whole corps, more perhaps than any other in the army, that when officers study the characters of their men, and use in governing them the knowledge so acquired, they are amply rewarded by the result, and need no coarser discipline." Sergeant Manning, who worked under Lieut. Portlock through the whole period of his service on the Irish survey, was chosen as the non-commissioned officer best fitted to take charge of a party sent in 1848 to the Cape of Good Hope, to verify, under direction of Mr. (now Sir Thomas) Maclear, the base measured by Lacaille nearly a century before.

Of the great value of the Irish survey in connexion with the geology, archæology, statistics, and industrial resources of Ireland, this is not the place to speak. Suffice it to say that when the time came for drawing up a Report on the subject, Lieut. Portlock proved himself not less able as a geological than as a geodetical observer. His separate Report on the Geology of Londonderry has been pronounced by high authority to be "a perfect model for fidelity of observation and minute attention to phenomena." It is safe to affirm that the name of Portlock will ever be most honourably associated with the history of the Ordnance Survey of Ireland.

In 1843 Captain Portlock was ordered to Corfu on the ordinary duties of his corps. In the comparative leisure which he then enjoyed he wrote papers on the geology and natural history of the island, and on professional subjects. Some of these were published in the Reports of the British Association, the Annals of Natural History, and Journal of the Geological Society. The Association voted him a grant "for the Exploration of the Marine Zoology of Corfu," the results of which he embodied in two papers subsequently published. In these again we have evidence of his activity of mind and accuracy of observation.

Recalled to England in 1847, Major Portlock was stationed first at Portsmouth, and afterwards, as Lieut.-Colonel, at Cork. From this time the literature of his profession and scientific study engaged much of his attention. The annual volumes published by the British Association contain papers from his pen; and besides contributions to the *Professional Papers* of the corps, he wrote the articles "Geology and Geodesy," "Galvanism," "Heat," "Palæontology," and an Appendix on Gun-Cotton for the *Aide-mémoire*, and the Treatise on Geology in Weale's Rudimentary Series. Others of his papers appear in the Journal of the Geological Society of Dublin, of which Society he was four times President.

In 1851 Lieut.-Col. Portlock was appointed Inspector of Studies at the Royal Military Academy, Woolwich, in which place he helped forward measures for improving the scientific character of the system of education, and increasing its efficiency generally; and during this period he wrote the articles "Cannon," "Fort," "Gunnery," and revised the article "War" for the 8th edition of the 'Encyclopædia Britannica,' besides translating for the new series of *Professional Papers* a work on Gunpowder (from the French), and a treatise on Strategy (from the Italian). He wrote also a memoir of his former chief, Major-General Colby, a publication honourable alike to the subject and the author. To all this must be added the two Addresses delivered by him as President of the Geological Society in 1857 and 1858, which, in the words of an eminent authority, are characterized by "faithful and elaborate research."

After resigning his appointment at Woolwich, and holding the command for a few months at Dover, Major-General Portlock became in 1857 a Member of the Council of Military Education, in whose proceedings, as might have been expected, he took an active and earnest part. His sentiments with regard to the objects in view may be gathered from a memorandum drawn up by one of his colleagues, who writes, "General Portlock's opinions on the questions presented to him as a Member of the Council were in all cases those of the most forward advocates of education. He looked upon competition as the great principle upon which public appointments should be made, nor did he shrink from the inevitable social results which such a change would involve. Education, combined with good morals, he regarded as constituting a paramount claim to the rank of gentleman. He was therefore a warm advocate of the system of open competition as applied to the elections into the Royal Military Academy of Woolwich; nor did he share the apprehension, which has been very frequently expressed, of a consequent lowering of the social position of the officers of the two great scientific corps.

The weakness and infirmities of advancing years were borne by General Portlock with a spirit not less calm and patient than that which animated him through the hardships of the Ordnance Survey. He retired to Lota, a pleasant spot near Dublin, and there died on the 14th February, 1864. He was elected a Fellow of the Royal Society in June 1837, and was a member of other metropolitan and provincial Societies. The honorary

degree of Doctor of Laws was conferred on him by Trinity College, Dublin, in 1857."

This brief notice of one who was for twenty-seven years an honour to the Society, may be fittingly closed with a few words of affectionate testimony by a brother officer, to whose Memoir we are indebted for much of the foregoing. "The characteristics which shone forth in Portlock during his well-spent life," writes Major-General Sir Thomas Larcom, "whether as a soldier, a geographer, or a geologist, were—undaunted courage in facing difficulties, Spartan endurance and invincible perseverance in overcoming them. Endowed, when in the zenith of his career, with a frame and nerves of iron, he exhibited such a vast power of continuous labour, that he achieved every object he had in view; while great ability, and a pure love of knowledge, were in him guided and governed by the highest sense of honour and moral rectitude."

Dr. Archibald Robertson was born at Cockburnspath in Scotland, on the 3rd of December, 1789. He studied medicine at Edinburgh, and in 1808 entered the Naval Medical Service. After some years of active employment in Europe and America, he on the termination of the war resorted again to Edinburgh for the further prosecution of study, and took his degree of M.D. in that University in 1817. He then settled as a physician in Northampton; and although for more than a twelvemonth he did not receive the encouragement of a single fee, he held on to the position he had taken, and was soon rewarded by large and lucrative employment, his success being promoted and assured by his being in 1820 elected Physician to the Northampton Infirmary. After a long and prosperous professional career, and the acquisition of a handsome independence honourably earned, he in 1853 resolved to withdraw himself from the labour of active practice. He accordingly left Northampton, and passed the rest of his life in retirement in the west of England.

Dr. Robertson was a man of considerable literary accomplishment, and, before his time became engrossed by practice, he was in the habit of writing literary articles in some of the journals and reviews of the day. He contributed two short articles on professional subjects to Forbes's 'Cyclopædia of Medicine.' He was elected a Fellow of the Royal Society on the 11th of February, 1836.

Both as a physician and as a member of society, Dr. Robertson was highly esteemed. His death took place at Clifton, on the 19th of October, 1864.

Giovanni Antonio Amedeo Plana, descended from an ancient and distinguished family of Guarene in Piedmont, was born at Voghera, on the 8th of November, 1781. In 1800 he entered the Polytechnic School of Paris, where he so greatly distinguished himself that, on the 23rd of May, 1803, he was appointed Professor in the Artillery School of Alessandria.

On the 28th of November, 1809, he presented to the Academy of Turin a paper, entitled "Équation de la courbe formée par une lame élastique quelles que soient les forces qui agissent sur la lame," the first of a series of papers offered to the same Academy, far too numerous to be recorded in the present notice. On the 15th of March, 1811, on the recommendation of Lagrange, he obtained the Professorship of Astronomy in the University of Turin, and on the 5th of March, 1813, became Director of the Observatory. After the Restoration, the king, Victor Emmanuel I., who took a personal interest in the progress of astronomy and frequently sent for Plana to explain various celestial phenomena, augmented the income of the Observatory, and transferred it from the house of the Academy to a better situation on the west tower of the north face of the Palazzo Madama. During the years 1821, 1822, 1823 he was associated with Carlini in the operation of measuring an arc of parallel in Savoy and Piedmont. The results were published in 1825, under the title "Observations géodésiques et astronomiques pour la mesure d'un arc de parallèle moyen." In 1828 the authors received from the Institute the Lalande prize for the astronomical part of their joint work. In 1832 he published his 'Théorie du mouvement de la Lune,' in three large quarto volumes. This he regarded as the most important of all the labours of his life. For this work the Copley Medal was awarded to him in 1834, and the Gold Medal of the Astronomical Society in 1840. In announcing the latter award, Sir John Herschel, President of the Society, made the following quotation from the "Discours préliminaire" of the 'Théorie de la Lune'—"Je n'ai pu me faire aider par personne; j'ai dû traverser *seul* cette longue chaîne des calculs, et il n'est pas étonnant si, par inadvertence, j'ai omis quelques termes qu'il fallait introduire pour me conformer à la rigueur de mes propres principes,"—adding, "When we look at the work itself there seems something awful in this announcement."

In 1822, on the occasion of the appearance of his "Mémoire sur les mouvements des fluides qui recouvrent une sphéroïde à peu près sphérique," he was elected a Corresponding Member of the Institute, and in 1860 one of the eight Foreign Associates. In December 1851 he became President of the Royal Academy of Turin. He was elected Foreign Member of the Royal Society in 1827. He received from his own king the title of Baron, and was created a Senator on the formation of the Senate in 1848.

He delighted in the classic poets, and was not more remarkable for the accuracy and elegance of his mathematical investigations than for the precision of his style in writing. He was in the habit, it is said, of bestowing extraordinary care on the composition and correction of his works.

On the 6th of January, 1864, he read a paper before the Royal Academy of Turin, entitled "Mémoire sur les formules du mouvement circulaire, et du mouvement elliptique libre autour d'un point excentrique par l'action d'une force centrale." This was his last work. He died at Turin on the 20th of January, 1864, leaving a widow (Lagrange's niece) and a daughter.

The death of his only son, on the 27th of March, 1832, called forth the expression of grief which concludes the Introduction to the 'Théorie de la Lune.'

HEINRICH ROSE was born on the 6th of August, 1795, at Berlin, where his father, son of the discoverer of the fusible alloy known by his name, was Pharmacist and Assessor of the Superior Medical College. His father died in 1807, leaving behind him a widow and four young boys. H. Rose studied Pharmacy first in Dantzic, where he experienced the horrors of a siege, and nearly lost his life by typhus fever. He served in the campaign of 1815, together with his three brothers, of whom one is Professor Gustav Rose, the distinguished Mineralogist of Berlin. On the conclusion of the war he continued his studies in Berlin, working in Klaproth's laboratory during the summer of 1816. In September 1816 he entered the Pharmacy of Dr. Bidder of Mitau. About the end of 1819 he went to Stockholm, where he worked for a year and a half in the laboratory of Berzelius, who recommended him to devote himself to the teaching of chemistry as a profession. On quitting Stockholm he resided for some time at Kiel, where he wrote his Dissertation "de Titanio ejusque connubio cum oxygenio et sulphure," and took the Degree of Doctor of Philosophy. In the summer of 1822 he obtained the sanction to become a private teacher in the University of Berlin, and began a course of lectures on practical analytical chemistry in the autumn of the same year. He was appointed Extraordinary Professor in 1823, and Ordinary Professor of Chemistry in 1835. He was elected a Member of the Berlin Academy in 1832, Foreign Member of the Royal Society in 1842, Corresponding Member of the Institute in 1843, and was invested with the Prussian order of *pour le mérite*.

His memoirs on inorganic chemistry and chemical analysis, a department in which he stood unrivalled, to the number of nearly, if not quite, two hundred, are contained principally in Gilbert's and Poggendorff's 'Annalen.' The results of his researches in analytical chemistry are embodied in his 'Handbuch der analytischen Chemie,' which came out in one volume in 1829. A second edition, in two volumes, was published in 1831, a fourth in 1838, a fifth in 1850, the sixth (so thoroughly revised that it should be regarded as a new work) was published in French, at Paris, in 1861. In forming an estimate of the labour expended in preparing this voluminous treatise, it must be remembered that each precept is the result of an experiment (frequently of a series of experiments) made by the author. During the last years of his life he was engaged in writing an elementary treatise on analytical chemistry, about thirty sheets of which were printed during his lifetime. For this work also a large number of experiments were made in his laboratory. His activity and industry increased with advancing age. A year before his death he was heard to exclaim, "I have at most only a few years to live, and so much remains to be done!" During the latter

part of his life his only recreation was a long walk taken late in the evening, in all weathers, throughout the year. He was the first person in all Germany who established a class of working pupils. He received them in his private laboratory without fee, providing at his own cost most of the apparatus and all the reagents they required.

He was spared the pain of feeling the approach of bodily and mental infirmity. He lectured in full possession of all his faculties only eight days before his death, and he was confined to his bed only seven days. On the 27th of January, 1864, he asked for writing-materials to correct some proof sheets, saying that he felt well, and that he could now leave his bed. That afternoon he died, of inflammation of the lungs. He left behind him a widow, his third wife, and a grandchild, the daughter of Professor Karsten. Her mother, H. Rose's only child, died some years since.

Friedrich Georg Wilhelm Struve was born at Altona on the 15th of April, 1793. He was the fourth son of Dr. Jacob Struve, Director of the High School of Altona. His mother was the daughter of Pastor Stinde, Chaplain to Peter III., Emperor of Russia. In order to avoid the French conscription, he went in 1808 to the University of Dorpat, where his elder brother Carl was a Classical Lecturer. At first he devoted himself to Philology, a study in which he delighted to the end of his life. He supported himself partly by private tuition in the family of M. de Berg, and partly on some pecuniary assistance afforded him by the University on the recommendation of the elder Parrot, who had discovered Struve's promise of future eminence. In 1811, after taking his first degree in Philology, he commenced the study of Astronomy under Huth, who permitted him the free use of the few instruments contained in the Observatory at that time; and in August of that year he verified by observation the conclusions of Sir William Herschel respecting the angular motion of the two stars composing Cas or. In the autumn of 1813 he took the degree of Doctor of Philosophy, the title of his thesis on that occasion being "De geographicâ speculæ Dorpatensis positione." In November 1813 he was appointed Extraordinary Professor of Mathematics and Astronomy, and, after the death of Huth, Ordinary Professor, and Director of the Observatory. During the years 1816–19 he surveyed and mapped Livonia at the request of the Economical Society of that Province, the only instrument employed by him in the survey being a 10-inch sextant by Troughton.

In 1821 the Observatory of Dorpat was supplied with a meridian-circle by Reichenbach and Ertel, and in 1824 with an equatorially mounted refractor, of 9 Paris inches aperture and 160 Paris inches focal length, by Fraunhofer. The principal results of the observations made by Struve at Dorpat during the years 1814–1838 are given in the works entitled "Observationes astronomicæ institutæ in speculâ Dorpatensi, 1817–1839," "Catalogus 795 stellarum duplicium, 1822," "Catalogus novus stellarum duplicium et multiplicium, 1827" [in the introduction to this

work it is incidentally noticed that on one occasion he had observed uninterruptedly for eight hours in a temperature of $-25°$ C.], "Stellarum duplicium et multiplicium mensuræ micrometricæ, 1837," "Stellarum fixarum imprimis compositarum positiones mediæ, deductæ observationibus meridianis a 1822 ad 1843 in speculâ Dorpatensi, 1852," "Beobachtungen des Halley'schen Cometen bei seinem Erscheinen im Jahre 1835, auf der Dorpater Sternwarte angestellt, 1839."

In the spring of 1839 he left Dorpat to assume the Directorship of the Observatory of Pulkowa, built in accordance with his own plans, and furnished with instruments contrived and executed under his own directions. An account of the building and instruments is given in his "Description de l'Observatoire central de Pulkowa, 1845." In 1843 he published his "Catalogue de 514 étoiles doubles et multiples, &c., et Catalogue de 256 étoiles doubles principales où la distance des composantes est de 32″ à 2′ &c.," and "Sur le coefficient constant dans l'aberration des étoiles fixes déduit des observations qui ont été exécutées à l'observatoire de Poulkova." In 1847 he published "Etudes d'Astronomie stellaire."

Struve devoted a portion of the summer for many years to the vast undertaking of measuring an arc of the meridian of 25° 20′ from Fuglenaes on the Arctic Ocean, lat. 70° 40′, to Ismail on the Danube, lat. 45° 20′. This work may be considered the principal labour of his life: he was assisted in it by General Tenner and the astronomers Selander and Hansteen. It lasted thirty-seven years, and was completed in 1853. An account of the measurement is given by Struve in "Breitengradmessung in den Ostseeprovinzen Russlands ausgeführt und bearbeitet in den Jahren 1821 bis 1831," in "Exposé historique des travaux exécutés jusqu'à la fin de l'année 1851, pour la mesure de l'arc du méridien, &c., 1860," and in "Arc du méridien de 25° 20′ entre le Danube et la mer Glaciale, 1860."

Besides the works already mentioned, he is the author of many separate treatises, and of papers in Bode's 'Jahrbuch,' the 'Zeitschrift' of von Lindenau and Bohnenberger, von Zach's 'Correspondance Astronomique,' and the 'Bulletins' and 'Mémoires' of the Imperial Academy of St. Petersburg.

In 1858 he was attacked by a severe illness, for which rest from work and travelling were prescribed. These remedies, however, failed to remove the consequences of his malady. In 1862 he retired from the post of Director of the Pulkowa Observatory, and was succeeded by his son O. W. Struve. He then went to live with his family in St. Petersburg, occupying the remainder of his life with the subject of double stars. On the 4th of November he felt indisposed; his strength failed rapidly; and he died on the morning of November the 23rd, 1864.

He was elected Foreign Member of the Royal Society in 1827, and in the same year one of the Royal Medals was awarded to him for his 'Catalogus novus Stellarum duplicium.' He received the Gold Medal of the Royal Astronomical Society in 1826, "for his important researches on the

subject of multiple stars." His name appears in the list of Associates in the first volume of the 'Memoirs of the Astronomical Society.' In 1833 he was elected Corresponding Member of the Institute.

Struve married twice; he had twelve children by his first wife, of whom eight survive, and eight by his second wife, now his widow, of whom four survive.

Much of the present notice has been derived from a very comprehensive sketch of Struve's life and labours in the Proceedings of the Astronomical Society, by the Rev. C. Pritchard.